

Appendix 1-3: Authors' Responses to Peer-Review Panel and Public Comments

During September–October 2012, the peer-review panel posted their comments on draft Volume I on the 2013 SFER WebBoard at www.sfwmd.gov/webboards (see Appendix 1-2). This appendix includes authors' responses to panel comments provided on the WebBoard. With the exception of spell check and reformatting some information for better readability, this appendix was not edited by the SFER production staff and appears verbatim as posted on the WebBoard. No public comments on draft Volume I were received on the WebBoard.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 3A

Paul Julian¹, Grover G. Payne² and Shi Kui Xue

Level of Panel Review: Accountability

Reviewer: V. Novotny (AA)

Posted: 10/9/12 @ 1:38 PM EST

***Comment #1:** Page 3A-2 - lines 38 to 43 Alkalinity and pH excursions could be of natural origin which could be disposed of by a Use Attainability Analysis (UAA). It is not clear from the text, although it is implicit, whether the alkalinity excursions were on the low side. A statement why low alkalinity is a concern could be added.*

Response #1: This section is purely used to provide a quick summary of this water year's water quality status and trend. Additional information and follow-up discussion is provided within the main body of the text under the "Water Quality Criteria Excursion Analysis" section, "Alkalinity and pH" subsection.

***Comment #2:** Lines 52 to 55. The sulfate excursions should be of concern and this will be discussed in the review of Chapter 3B. High sulfate concentrations penetrating into sediments release phosphate into solution. On the other hand, low sulfate (but not extremely low) promotes formation of methyl mercury which is a highly toxic bio-accumulative constituent. Because enough pioneering knowledge has been developed by SFWMD scientists and their consultants, the State of Florida should seriously consider developing a site specific sulfate standard that would be related also to dissolved organic carbon (DOC) and, maybe, to other constituents (e.g., type of DOC). This will also be discussed and proposed in more detail in the review of Chapter 3B.*

Response #2: Noted. Due to the nature of the comments and the substance of Chapter 3B, comments associated with sulfate and mercury methylation will be addressed in the peer review responses for that chapter.

***Comment #3:** Lines 61-70. Total phosphorus data indicate that the TP standard of 10 µg/L is generally met in the EPA Park and very close to being met in the Refuge Area. The WY 2012 and 2013 SFERs provides a more positive outlook regarding the trends and chances of meeting the standard in the near future.*

Response #3: Noted. The data indicate conditions within the EPA continually improve and additional restoration projects aimed at improving water quality within the Everglades are expected to further this trend.

***Comment #4:** Page 3A 3- lines 84 – 86. The district collects and analyzes nitrogen separately for TKN (Total Kjeldahl Nitrogen = organic N and Ammonium) and nitrate/nitrite N. If possible, the authors should provide also separate information for these constituents in the report and Summary because these constituents, in some aspects, affect water quality and eutrophication*

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differently. New knowledge on nitrate/nitrite eutrophication effects will be presented in the subsequent section of this review.

Response #4: Noted. Thank you for this suggestion. The authors will consider adding this analysis in the following year's report.

Comment #5: *Pages 3A– 3A8 The monitoring program was established years ago and has been providing excellent data bases.*

Response #5: The authors agree.

Comment #6: *Page 3A-9 line 223. The readers generally are not familiar with the concept of “fatal” qualifiers included in the Florida Administrative Code. A brief explanation of the meaning of “H, J,K,N,O,V,Q,Y, or ?” would be very helpful. Also on page 225, there is no way pH can be measured as greater than 14. This could be stated better as “if a typographical error such as pH greater than 14 has been found....”*

Response #6: Text will be added to explain a fatal qualifier. “Fatal qualifiers are standard data qualifiers used by both laboratories and field samplers to indicate that the quality or accuracy of the data may not be suitable for statistical analysis. As such data qualifiers can be used to indicate that a sample(s) was not properly preserved (qualifier Y), sample was not analyzed within the acceptable window (qualifier Q), the analytical analysis was flawed (qualifier J, K, N, O, V, and ?), or data were estimated with a lower accuracy method (qualifier H).”

Comment #7: *Page 3A-10 - Water quality excursion analyses lines 263 – 261. In last year’s report review this reviewer pointed out potential problems with the estimates of water quality concerns. The 10% excursion limit for “concerns” is generally used in state water quality reporting to the US EPA and Congress under Section 305 (b) of the Clean Water Act and for constituents such as DO could lead to accepting “fatal” but within the 10% excursion limit as only “a potential concern”. This concern should be repeated in this review but it is not aimed at the SFWMD scientists who are constricted by the federal Section 305d guidelines adapted by most states. This evaluation leads to a preliminary list but may not be used to developing TMDLs under Section 303 (d) of the Act which are based on more stringent allowed exceedances for parameters such as DO, unionized ammonia and priority pollutants that would include pesticides. Generally, if a parameter consistently fails the 305(b) frequency criterion the water body should be put on the 303(d) list leading to TMDL or UAA. In general, the approach based on the frequency distribution in the 2013 SFERR is correct.*

Response #7: Unlike most other parameters, dissolved oxygen (DO) is not a direct pollutant. Instead, it is a secondary response parameter that reflects changes in other pollutants or physical or hydrologic changes in the system. The Florida Department of Environmental Protection recognizes that DO impairments in phosphorus-impacted areas are related to biological changes caused by phosphorus enrichment. Phosphorus concentrations in excess of the numeric criterion produce a variety of system changes in the Everglades that ultimately depress the DO regime in the water column. The South Florida Water Management District is actively implementing a comprehensive restoration program to lower TP concentrations within the phosphorus-impacted portions of the Everglades Protection Area. DO concentrations at the nutrient impacted sites are expected to continue to improve as phosphorus concentrations in surface water and sediment are reduced and biological communities recover.

Comment #8: *Page 3A-13. Lines 327-335. The concern with sulfate concentrations is warranted. Fish and other organisms residing in some parts of the Everglades system have very high mercury tissue content. The 2012 WY apparently had sulfate concentrations 75% higher than those in 2011 in the most of the interior marsh stations. The sulfate content in water is linked to that in pore water of sediments which causes two problems:*

A. Reduction of sulfates to sulfides in anoxic/anaerobic sediments by sulfate reducing bacteria (SRB) releases iron bound phosphate into solution and ultimately into overlying waters. This

phenomenon is causing the managers to calls to develop measures to reduce sulfate concentrations. Sulfides in the sediment pore water are toxic and prevent formation of methyl mercury.

B. However, in some parts of the EPA and Refuge Areas, sulfate concentrations are low, below 10 mg/L. The research reported in Chapter 3B of the 2012 SFER indicated that sulfate concentrations in water above the sediment between 1 to 9 mg/L reduce the sulfide toxicity to SRB and lead to formation of methyl-mercury by the same SRB. The SFWMD scientists and consultants have now enough excellent information for developing a scientific site specific criterion that could lead to sulfate TMDL and abatement focused on reducing sulfate loads to levels that would both control eutrophication and prevent formation of methyl mercury.

Response #8: Noted. Due to the nature of the comments and the substance of Chapter 3B, comments associated with sulfate and mercury methylation will be addressed in the peer review responses for that chapter.

Comment #9: *Page 3A-16 Dissolved Oxygen. Repeatedly, like in the previous reviews, this review again must point out the dichotomy of the Everglades DO site specific alternative criterion which, apparently, was developed from long term observations of DO concentrations. It is a well known fact that wetlands, including EPA, are naturally dystrophic and exhibit low DO. The SSAC criterion is actually a harmonic model which calculates “unimpacted” DOs from temperature and annual and diurnal time. It was pointed out in the previous reviews that some of these calculated SSAC were at or below the lethal values for quality fish and using annual averages for the assessment does not make much sense. It was also pointed out previously that fish kills occurred in the EPA, lastly in 2010 but apparently the last fish kill was not caused by low DO. The previous reviews recognized the fact that the authors must adhere to the current standards and guidelines and not to get involved in the debate on the validity or deficiencies of the current SSAC imposed by the state or federal regulatory agencies. Hence, as in the past, this comment on the problem with the DO assessment is not aimed at the authors; it is directed towards the agencies that developed and/or accepted the harmonic formula and its application to averaged DO concentrations.*

Response #9: Noted. Thank you for your comment.

Comment #10: *Page 3A-16 to 17 – Alkalinity and pH. If the inflow to the refuge interior is dominated by atmospheric inputs it is obvious that the refuge and EPA water will have naturally low pH and alkalinity. Typically, natural pH of rain is around 5.7 and has very low alkalinity. Again, the reviewer agrees with the report that possible excursions of alkalinity are not of a major concern; they may be natural. Similarly occasional lower pH is most likely natural due to the dominance of the rainfall as the main hydrological input in the protected areas. Natural pH of rainfall is less than 6.*

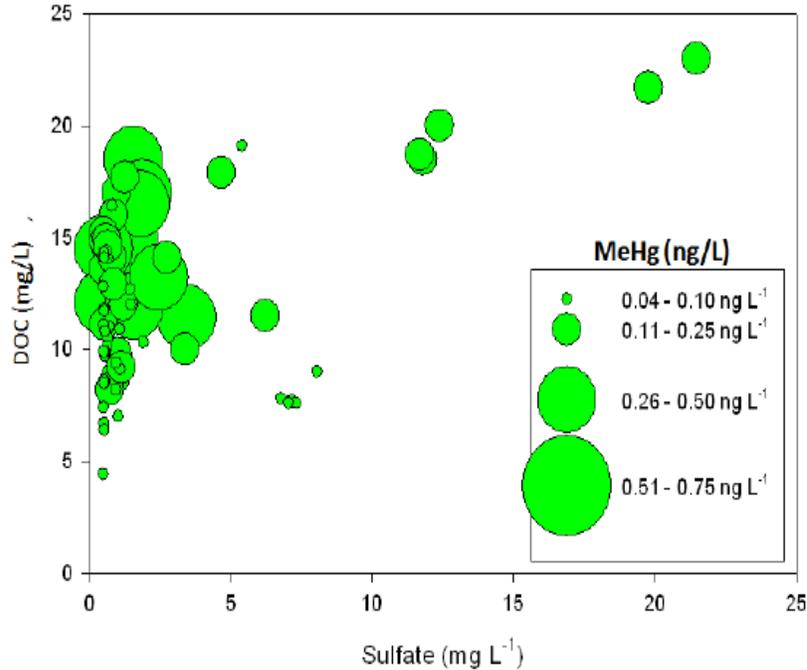
Response #10: Noted. Thank you for your comment.

Comment #11: *Page 3A-17 – Specific conductivity. The Refuge rim stations as well as some WCA 2 inflow stations had a problem with high salinity (conductivity) caused mainly by increased pumping of salty water from canals. In general the specific conductance excursions decreased in the EPA interior areas. It is logical that if a significant portion of the input is by atmospheric wet deposition, enough dilution may be available to bring the interior salinity to acceptable levels; however, the high salinity in Refuge and WC-2 is a concern. Because the canal discharges may continue in the future, the SFWMD should develop a mass balance model and discharge management that would avoid salinity problems in the affected areas. In WCA – 3 and Park the interior salinity is normal.*

Response #11: Noted. The authors will take the reviewer’s suggestions into consideration.

Comment #12: *Page 3A20 and 21– Sulfate. This reviewer will address the sulfate issue again in his review of Chapter 3B (“A” review). Table 3A-2 indicates that high sulfate concentrations*

were pointed out on page 3A-17 as being 75% higher than in the preceding water year. This means that more dissolved phosphorus will be released into the pore water of the sediments and, subsequently, potentially in the water above the sediment. However, in the interiors of WCA-2, WC-3 and EPA, 25th percentile to median values were between 1 to 5 mg/L which is the “Goldilocks” window at which methyl-mercury is suspected to be formed. It appears that all three could be adversely affected by the sulfate concentrations in the 1 to 5 mg/L sulfate window (see Figure below).



This figure taken from the 2012 SFER shows the 1-5 mg/L sulfate window in which methyl mercury can be formed at higher concentrations if the dissolved organ carbon (DOC) is between 10 – 20 mg/l , (i.e., the water body is organically enriched by algae development). The methyl mercury formation is only mildly affected by mercury inputs because the atmospheric Hg deposition could be sufficient.

Response #12: Noted. Due to the nature of the comments and the substance of Chapter 3B, comments associated with sulfate and mercury methylation will be addressed in the peer review responses for that chapter.

Comment #13: Pages 3A- 22 –3A-24 Pesticides. Unlike the previous report which identified only atrazine as a pesticide of concern, during WY 2012 nine pesticides or pesticide breakdown products were detected at concentrations above the minimum detectable level (MDL) but none of them at levels US EPA priority criteria.

Response #13: Noted. Thank you for your comment.

Comment #14: Pages. 25 - 31 Phosphorus. Phosphorus has been recognized as a limiting nutrient for the Everglades (EPA). To protect the biota and limit potential eutrophication a criterion of 10 µg/L of Total Phosphorus was designated for the areas. As in the previous year report, this section on phosphorus presents again the trends of long term geometric averages of TP in the various locations of the EPA water bodies. These long term averages were influenced in the water years 2005 by extreme meteorological events from which the system was recovering in the subsequent, mostly dry years. It is expected (lines 599 – 605) that as the structural and nonstructural Phase II BMPs for agricultural pollution and STA measures for runoff pollution

load controls will be implemented, the results will be less affected by climatic conditions and the results of long term restoration will become more clear.

Figure 3A-9 and 3A-10 document the trends. Figure 3A-9 reports annual P concentrations for refuge and WCA-2 areas. In the refuge area the P concentration trend in the inflow is downward and the interior the P concentrations have been stabilized around the goal of 10 µg/L since 1994 with a local spike in 2005. In WCA-2 the annual P concentration in the inflow have also been decreasing since 1994 and, as a result, the interior concentrations are now actually below the goal of 10 µg/L. Same trends and general compliance with the 10 µg/L goal were reported in Figure 3A-10 for WCA-3 and ENP areas.

Maximum concentrations of the TP in the inflow were around 40 µg/L in the WY 2012; however, they reached a maximum of 200 µg/L. In the ENP the P concentrations are well below the goal.

These trends can be characterized as good news. Nevertheless, the last several years were affected by drought, resulting in lower P inputs into the system. The refuge site is affected by the canal inputs which were limited in the WY year. The effects of BMPS and structural measures will be noticeable during wet years.

Response #14: Noted. Thank you for your comment.

Comment #15: *Pages 3A-31-32 – Orthophosphate. Geometric means of orthophosphate concentrations reported in Table 3A-5 are well below the goal. However, maxima reported in the table were very high in the past but not in WY 2012. Is there an explanation? Is it the effect of the 2005 hurricane year that impacted the 2005 – 2011 period. This should be stated in the report.*

Response #15: Text will be included providing potential explanations to the low range of values. “Additionally the reduced range of values experienced during WY2012 could be the result of a relatively dry wet season (i.e., reduced rainfall), reduced TP loads into the EPA as well as further optimization of BMPs.”

Comment #16: *Pages 3A – 33- Total Phosphorus Load. This section is revealing and explains partially the downward trend in concentrations even when the BPs and STAs are essentially in the first Phases of implementation. The long term atmospheric phosphorus load to the WCAs only was reported as ranging between 107-147 metric tons while the total load from land sources is 36.7 metric tons. The total loads are correlated to the flow input (Figures 3A-12 and 3A-13). The flow is related also to atmospheric deposition. This poses a managerial and also political dilemma in pushing for completion of the remedial program; however, it can be stated that the atmospheric deposition is also favorably affected by agricultural BMPs that reduce both water and wind (atmospheric) erosion soil erosion.*

In the review of Chapter 4 it will be pointed out that the largest and the only phosphate surface (Bone Valley) mine is located in the vicinity of the northern watersheds of the Everglades. Surface mines are notoriously known for being a large source of dust entering atmospheric currents. Most likely, the effect of phosphate mining in the vicinity of the Everglades’ watershed may not be known.

Regarding units on the figures; while load is expressed in SI (metric) units the flow is in archaic US units. Please provide a conversion from acre-ft to m³ and also Table 3A-6 below the figures and table. The metric equivalent of acre-ft is simply m³ (or 1000 m³) or 1 acre-ft = 1233.5 m³. Unit hectare-meter does not exist in the SI technical literature. The adherence to SI units was requested also in the last year review.

Response #16: Noted. The long-term atmospheric phosphorous load to the WCAs was reported as ranging between 107-143 metric tons. It was calculated based on the range, expressed spatially as 20 t o 35 m illigrams per square meter per year (mg/m²/yr); the total annual load from land sources i s 36.7 m etric t ons, wh ich was calculated f rom al l f lows and monitored T P concentrations. Furthermore, the effect of phosphate mining in the vicinity of the E verglades’ watershed is not known. Text will be added discussing atmospheric deposition.

A footnote in the table will be revised to include the acre-feet to cubic meters conversion. The use of the acre-ft unit convention is often used when handling large volumes of water. These units were used in other S FER chapters and have been used since the inception of the S FER/Everglades Consolidated Report. Also, the unit is widely used by the SFWMD and its constituents.

Comment #17: Pages 3A – 34- Total Phosphorus Load. The reviewer agrees with the assessment stating, based on the data provided in the report and appendix, that unimpacted portions of each WCA and the entire EPA were in compliance with the 10 µg/L phosphorus criterion. However, the impacted portions failed at least one of the compliance tests. The district should identify the reason and readjust the abatement program if needed.

Response #17: Impacted portions of the marsh are expected to fail due to historical phosphorous enrichment and high soil total phosphorous concentrations. As noted in the “Total Phosphorous Criterion Achievement Assessment” section and displayed in Appendix 3 A-6 Figure 1, the general trend of surface water total phosphorous concentration in the impacted portions of in the marsh is decreasing, therefore conditions are improving and the majority of the relatively higher phosphorous impacted stations are influenced by canal water (i.e., X1 and Z1 in the Refuge). With that in mind the District is implementing a regional water quality plan to improve water quality and meet the compliance requirements. The detail project features are described at:

http://www.dep.state.fl.us/water/wqssp/everglades/docs/sta/restoration_strat_regional_plan.pdf

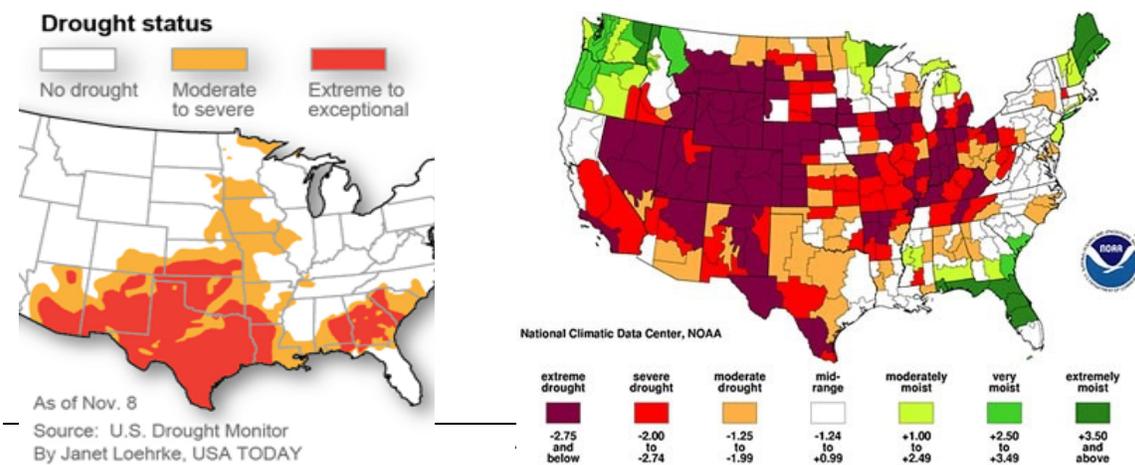
Comment #18: Pages 3A-37-40 – Total Nitrogen Concentrations. Nitrogen is not a limiting nutrient in the Everglades (it is in the Saint Lucie estuary) but common sense dictates that both TP and TN loads should be reduced to protect the Everglades protected areas. Generally, most BMPs reduce both nitrogen and phosphorus.

Response #18: Noted. Thank you for your comment.

Comment #19: Table 3A-37 shows that N concentrations in WY2012 were significantly lower than those for the multiple year averages of the previous periods. This could be misleading because the multiple year period may all contain an extreme climatic event (hurricane) that increased the mean when compared to the relatively dry eventless WY 2012. On the other hand, the reports suggested that one of the main sources of N, in addition to the agricultural inputs and Lake Okeechobee, is the organic matter produced naturally in the EPA wetlands.

Response #19: Noted. Thank you for your comment.

Comment #20: The overall water quality assessment documents a hopeful stabilization of the system, reaching a steady state, which in the protected areas is generally attaining the EFA very stringent goals typical for oligotrophic water bodies. The continuing question is whether or not this current situation is a sign of a permanent trend or a result of drought. It appears that Florida might not have been affected by the severe 2011 and 2012 droughts (see Figure below, 2011 drought is left and 2012 is right) but this question still should be kept open.



Response #20: Noted. Thank you for the comment.

Comment #21: *The most important revelation of the chapter, which might have been reported in the previous reports but was not discussed in the reviews, is the dominance of difficult (or impossible) to control atmospheric sources of phosphorus. This poses a dilemma to managers on one side but may not be a big problem because this chapter documents a compliance with the EFA phosphorus goals in the protected areas, with some exception in the inlet areas. Physically, the interior areas would be more affected by the atmospheric inputs while inlets may contain more surface inputs that are evidently attenuated in the water bodies by deposition. It is also hoped that the nonpoint pollution controls in the watershed may also bring about some reduction of atmospheric inputs of phosphorus. This problem is similar to that of the Great Lakes where more than 50 % of P input is also from atmospheric sources.*

Response #21: Noted. Thank you for your comment.

Comment #22: *The question of sulfate concentrations and controls and the effect on very high mercury concentrations in fish will be discussed in the review of chapter 3B. It has become clear that under the scenario of continuing atmospheric deposition of mercury, low sulfate concentrations are not desirable unless dissolved organic carbon is reduced below 10 mg/L. Unfortunately, the state-of-the-art of the fish mercury levels controls related to sulfate reductions is not yet fully developed and reduction of atmospheric deposition of Hg is uncertain and not achievable in a short run. However, because most of Hg deposition can be “blamed” on coal fired power plants, the recent trend to replace coal fired electricity production by natural gas and renewable sources may bring a needed relief. But, the recovery may last for years.*

Response #22: Noted. Due to the nature of the comments and the substance of Chapter 3B, comments associated with sulfate and mercury methylation will be addressed in the peer review responses for that chapter.

Comment #23: *The phosphorus loads from the Lake Okeechobee and other inflows may still be high and should be controlled mainly to provide improvement to the lake which is overloaded with phosphorus and not healthy. However, as stated above, surface sources to protected areas are smaller than atmospheric sources.*

Response #23: Noted. Thank you for your comment.

Comment #24: As stated in the last year review, the SFWMD must continue to be vigilant regarding the low DOs in the system. It is recognized that Florida and specifically EPA wetlands are generally naturally dystrophic which is reflected in the site specific DO standard; hence, excessive excursions may lead to fish kills and so metimes, but rarely, even meeting the DO standard may not provide enough protection.

Response #24: Unlike most other parameters, dissolved oxygen (DO) is not a direct pollutant. Instead, it is a secondary response parameter that reflects changes in other pollutants or physical or hydrologic changes in the system. The Florida Department of Environmental Protection recognizes that DO impairments in phosphorus-impacted areas are related to biological changes caused by phosphorus enrichment. Phosphorus concentrations in excess of the numeric criterion produce a variety of system changes in the Everglades that ultimately depress the DO regime in the water column. The South Florida Water Management District is actively implementing a comprehensive restoration program to lower TP concentrations within the phosphorus-impacted portions of the Everglades Protection Area. DO concentrations at the nutrient impacted sites are expected to continue to improve as phosphorus concentrations in surface water and sediment are reduced and biological communities recover.

As described in responses to the peer-reviewer's comment on the WY2011 and WY2012 Chapter, the DO site-specific alternate criteria (DO SSAC) was developed with public input and was approved by both the State of Florida and the U.S. EPA as fully protective of the designated use of the waterbody. Additionally the U.S. EPA in their approval of the DO SSAC stated that:

“The Everglades DO SSAC establishes a revised water quality criteria for the Everglades Protection Area (Water Conservation Areas, 1, 2A, 2B, 3A, 3B, the Arthur R. Marshall National Wildlife Refuge and the Everglades National Park), which remains classified and protected for all designated uses of Class III waters, including recreation and propagation and maintenance of a healthy, well-balanced population of fish and wildlife. Based on our review of the supporting information as provided by FDEP for the Everglades DO SSAC, it is the Environmental Protection Agency's conclusion that the requirements of the Clean Water Act and provisions of 40 CFR Part 131 have been met.”

As in previous peer-review comments for Chapter 3A regarding DO, no information to support concentrations allowed by the DO SSAC as being “unprotective” have been provided.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 3B

Donald M. Axelrad²

Level of Panel Review: Technical

Reviewers: O. Stein (AA); V. Novotny (A)

Posted: 10/19/12 @ 2:39 PM EST

The following Peer Reviewer Comment #1 for the 2013 Draft SFER was submitted as part of the review of **Chapter 3A**, “*Water Quality in the Everglades Protection Area*” - “AA” prepared by Vladimir Novotny. As this comment is more closely related to the subject matter in **Chapter 3B**, “*Mercury and Sulfur Environmental Assessment for the Everglades*”, the comment is responded to here.

***Comment #1:** Lines 52 to 55. The sulfate excursions should be of concern and this will be discussed in the review of Chapter 3B. High sulfate concentrations penetrating into sediments release phosphate into solution. On the other hand, low sulfate (but not extremely low) promotes formation of methyl mercury which is a highly toxic bio-accumulative constituent. Because enough pioneering knowledge has been developed by SFWMD scientists and their consultants, the State of Florida should seriously consider developing a site specific sulfate standard that would be related also to dissolved organic carbon (DOC) and, maybe, to other constituents (e.g., type of DOC). This will also be discussed and proposed in more detail in the review of Chapter 3B.*

Page 3A-13. Lines 327-335. The concern with sulfate concentrations is warranted. Fish and other organisms residing in some parts of the Everglades system have very high mercury tissue content. The 2012 WY apparently had sulfate concentrations 75% higher than those in 2011 in the most of the interior marsh stations.

The sulfate content in water is linked to that in pore water of sediments which causes two problems:

A. Reduction of sulfates to sulfides in anoxic/anaerobic sediments by sulfate reducing bacteria (SRB) releases iron bound phosphate into solution and ultimately into overlying waters. This phenomenon is causing the managers to call to develop measures to reduce sulfate concentrations. Sulfides in the sediment pore water are toxic and prevent formation of methyl mercury.

B. However, in some parts of the EPA and Refuge Areas, sulfate concentrations are low, below 10 mg/L. The research reported in Chapter 3B of the 2012 SFER indicated that sulfate concentrations in water above the sediment between 1 to 9 mg/L reduce the sulfide toxicity to SRB and lead to formation of methyl-mercury by the same SRB. The SFWMD scientists and consultants have now enough excellent information for developing a scientific site specific criterion that could lead to sulfate TMDL and abatement focused on reducing sulfate loads to levels that would both control eutrophication and prevent formation of methyl mercury.

Page 3A20 and 21– Sulfate. This reviewer will address the sulfate issue again in his review of Chapter 3B (“A” review). Table 3A-2 indicates that high sulfate concentrations were pointed out on page 3A-17 as being 75% higher than in the preceding water year. This means that more dissolved phosphorus will be released into the pore water of the sediments and, subsequently, potentially in the water above the sediment. However, in the interiors of WCA-2, WC-3 and EPA,

25th percentile to median values were between 1 to 5 mg/L which is the “Goldilocks” window at which methyl-mercury is suspected to be formed. It appears that all three could be adversely affected by the sulfate concentrations in the 1 to 5 mg/L sulfate window (see Figure below). This figure taken from the 2012 SFER shows the 1-5 mg/L sulfate window in which methyl mercury can be formed at higher concentrations if the dissolved organ carbon (DOC) is between 10 – 20 mg/l , (i.e., the water body is organically enriched by algae development). The methyl mercury formation is only mildly affected by mercury inputs because the atmospheric Hg deposition could be sufficient.

The question of sulfate concentrations and controls and the effect on very high mercury concentrations in fish will be discussed in the review of chapter 3B. It has become clear that under the scenario of continuing atmospheric deposition of mercury, low sulfate concentrations are not desirable unless dissolved organic carbon is reduced below 10 mg/L. Unfortunately, the state-of-the-art of the fish mercury levels controls related to sulfate reductions is not yet fully developed and reduction of atmospheric deposition of Hg is uncertain and not achievable in a short run. However, because most of Hg deposition can be “blamed” on coal fired power plants, the recent trend to replace coal fired electricity production by natural gas and renewable sources may bring a needed relief. But, the recovery may last for years.

Response #1: Thank you very much for your positive comments regarding the quality of the science evaluating mercury methylation processes. The FDEP and SFWMD feel that the approximately \$2.5 million combined annual expenditures by the State and Federal parties have yielded a significant amount of important information on this topic. However, in response to the reviewer comment that “the State of Florida should seriously consider developing a site specific sulfate standard... that could lead to (a) sulfate TMDL and abatement focused on reducing sulfate loads to levels that would both control eutrophication and prevent formation of methyl mercury (MeHg)” we reiterate the statement in our summary to Chapter 3B that “...the role of sulfate and DOM (dissolved organic matter) in the biogeochemical cycling of mercury within the Everglades is complex and confounded by other variables, and these complexities must be understood and quantified before an effective strategy to reduce MeHg levels through the control/management of sulfate and/or DOM can be designed and implemented.” Also, please note that the research needs to address those complexities are identified in the summary and main Chapter, and the FDEP and SFWMD remain committed to implementing the necessary research and monitoring to address those needs.

The remaining Peer Reviewer comments for the 2013 Draft SFER were submitted as part of the review of **Chapter 3B**, “Mercury and Sulfur Environmental Assessment for the Everglades”

Comment #2: This report presents, maybe for the first time, an extensive synthesis compiled from sub-reports of multiple authors who are members of a number of eminent institutes and agencies along with the team from the SFWMD...

It can be stated at the beginning, that this effort to identify the causes and solutions of the methyl mercury contamination, the probable causes, interactions among the parameters, impacts on biota, and possible remedies (if any) represents the most comprehensive scientific endeavor since the mercury problem was identified more than forty years ago...

The wealth of information presented in the report is overwhelming and it may take some time before it gets into the literature...

The results are also cutting edge and new, in some cases not previously published anywhere...

The report contains a wealth of past and recent literature for further study.

It is also evident that the authors of the chapter in the last few years, as the new evidence and knowledge has been gathered, have readjusted the new conclusions, as a good science should always do when new information is gathered.

Response #2: Thank you.

Comment #3: 9: *It appears that this year's report alters the reporting period from a water year as done previously to a calendar year. It is not clear why this was done and it also appears that prior year data was not adjusted for the change. While that might not have an effect on the conclusions, it certainly would adjust the statistical means, percentiles and outlier data should in the figures. Please explain the ramification of this change.*

Response #3: Calendar year reporting ensures that both ENP largemouth bass samplings per calendar year are included in the annual SFER, whereas if the Water Year was used there are times when the data from the second sampling would not be included in the publication. The reporting-year change has not altered the data or conclusions.

Comment #4: 229: *the basis of site inclusion or exclusion could be better explained.*

Response #4: The authors will revise the text to make the basis of site inclusion or exclusion clearer.

Comment #5: 247: *Are the 32 south Florida sites the entire Everglades area or a component, or a greater area. It is best to be consistent with pre-defined geographic locations.*

Response #5: The locations of the 32 sites are shown in **Figure 3B-4**; the figure caption text will be revised accordingly.

Comment #6: 273-283: *What is the typical lifespan of Gambusia? This could be a co-determinate of the influence of wet and drought years.*

Response #6: *Gambusia* may live 1-2 years but mortality is such that they typically live for 4-5 months. The purpose for measuring THg in *Gambusia* is to look for short term spikes in MeHg availability. We will redraft this section to be more explanatory.

Comment #7: 285-301: *There are clearly some lines missing in this paragraph making the meaning difficult to ascertain. The paragraph will be re-worded for clarity.*

Response #7: Yes, there are some lines missing in this paragraph, and we will add them and revise the paragraph for clarity.

Comment #8: 331-339: *It is not clear how the reported data supports the conclusion of a time trend.*

Response #8: We agree that the text should be revised to better reflect Brandon's (2011) observed increasing of THg concentrations in panthers (fur) in the BCNP (north and south of I-75 combined) between 1978 and 2007, and Cunningham's more recent data for (2008–2012), which indicates that THg in panthers from the BCNP remains elevated.

Comment #9: 342-388: *The POR for these data is similar to that of the fish studies. While the number of samples is clearly smaller, the level of statistical analysis lags behind that of the fish studies. A higher level of statistical analysis would likely enhance the ability to make broad inferences of the collected data.*

Response #9: The chapter indicates that: "The broad trend in the data is that mercury levels in great egrets (feathers) fell sharply from a high in 1997 (mean = 20.68 ppm) to a low in 2003 (mean = 2.15 ppm) (**Figure 3B-9**)." The editors agree with this statement due to the trends present in Figures 3B-9 and 3B-10, which strongly suggest a decline in Great Egret THg exposure from the mid-1990's to the present, and which corresponds with South Florida Hg emissions reductions.

I will request that the contributor consider additional statistical analyses, but please note that the author acknowledges that there is some uncertainty in the magnitude of temporal changes due to the relatively high variance in measurements of colonies from year to year and because the colonies sampled have changed over time due to birds abandoning sites, and sampling logistics.

Comment #10: Figs 3-B-9, 3B-10 and 3-B11. Fonts are hard to read, especially the legend of 3B-10.

Response #10: Revised figures will be incorporated in the final version of this chapter.

Comment #11: 397-452: It is not clear how the data supports the conclusion that wading birds are the primary vectors for Hg. Also the depth profiling data suggest that the impacted sites have always been higher in both parameters. Is this translocation of Hg and N through the profile, if so how is depth translated to time. The stated conclusions may be supportable but are not supported as presented. In general this section is too short to convince the reader of the stated conclusions.

Response #11: Regarding the matter of whether the data support the conclusion that wading birds are biovectors for mercury transport from marshes to tree islands where they roost and breed, we will add text to clarify this.

The reviewer is correct that Hg and N are translocated through the soil profile. The depth data were converted to soil strata age data using soil dating. Text will be added for clarification. The conclusions of this study are supported by both high Hg and N15 values in the impacted tree island soils. However, we will revise the text to provide the rationale. Text will be added for clarification for using N15 as a proxy for high trophic level predators such as fish-eating birds, and explain the technique for the determination of the age of the soil in cores.

Comment #12: 531-537: This is an important section but how the "tagging" can determine the Hg sources by geographic region and not source (coal versus auto etc.) is not described.

Response #12: CMAQ (Community Multiscale Air Quality) modeling involved using a total of 14 separate tags for evaluating source contributions to total Hg deposition at different locations within the CMAQ model domain. This included five tags explicitly for Florida sources (*viz.*, coal-fired electricity generating facilities, oil-fired electricity generating facilities, waste incineration, cement kilns, and all other remaining Florida sources). Six other tags were used to identify all remaining sources within the conterminous US within a given geographical region (*viz.*, Georgia, Alabama, Mississippi, Louisiana, Texas, and sources associated with the remaining states in the conterminous US). All other remaining sources within the CMAQ model domain (including parts of Canada and Mexico) also were tagged as a source category. In addition, soil emissions of mercury (which are bidirectional) were ascribed a tag, as were "background" emissions from sources outside the CMAQ model domain (and imported to CMAQ from the global model, ECHMERIT) (S. Sillman, personal communication, January 7, 2012). Text will be added to this section.

Comment #13: 614: How are ambient versus new species delineated?

Response #13: Isotope-enriched $^{199}\text{HgCl}_2$ and Me^{201}Hg were added to soil, floc or periphyton samples, representing the newly input Hg species, in contrast to the ambient Hg present in the samples.

Comment #14: 629: While synthesis is generally expressed thought this chapter, how this study relates to the previous one is not addressed and they appear to be closely aligned.

Response #14: The section, "Aquatic Cycling of Mercury in the Everglades Database Availability and Initial Analysis", was included to highlight the database which was only very recently made available online. By here announcing the database (14 years of Everglades sampling; 61 parameters and 281 sites), and showing some initial output, we hope to promote its use by researchers for testing hypotheses.

Comment #15: Figures 3B-17 and 3B-19: These show the maximum rate of MeHg formation occurs when the concentrations of sulfate in water are around 10 mg/L and, subsequently, the largest MeHg in the Mosquitofish occur at 5 mg/L. The bell-shape curve is more spread out than

that for LMB fish in the last year's Chapter 3B. Figure 3B-19 does not show the effect of dissolved organic carbon or aromatic organic carbon compounds.

Response #15: The bell-shape curve is more spread out in Figure 3B-17 because of varying water and soil chemistry across the 10 EPA study sites for which data to plot the curve were taken. Figure 3B-19 does not show the effect of dissolved organic carbon on MeHg concentration as per last year's Chapter (different contributors), but the editors will consider its inclusion in future versions of this chapter.

Comment #16: Table 3B-3. Values for median changes do not appear to match values for reported medians. How do these parameters relate if not directly?

Response #16: The median delta (changes) and the new median value do not necessarily match because the metric is the *median* and not the average. We will double-check these values however.

Comment #17: 869-888: It is not clear how connate water is separated from ambient values. If reductions in connate water are necessary, as suggested, how could this possibly be achieved? If connate water has always been there, how could it be "elevating" sulfate concentrations?

Response #17: The presence of saline water at various depths is characteristic of Florida's groundwater resources. Geologic and paleolimnological evidence indicates that only 5000 years ago, the Everglades was a brackish water system, which would have been characterized by high ionic content. Connate seawater contributions (from groundwater) of sulfate to surface waters are based on using chloride as a tracer (with the assumption that the chloride is marine in origin) and applying the molar ratio of sulfate to chloride found in marine waters to estimate the marine component of sulfate concentrations in waters exiting the EAA. Further evidence as to why the chloride is believed to be marine in origin may be found in Pollman (2012).

It has been hypothesized that water management activities (for irrigation, flood control, etc.) in the vicinity of the EAA are largely responsible for capturing saline groundwater, delivering it to the canal system and subsequently, to surrounding marsh areas. Because the Floridan aquifer is poorly transmissive in the region of the EAA (Miller, 1988), upward discharges of connate seawater were likely not significant until the C&SF Project made dramatic physical and hydrological alterations on the landscape.

An example of how the CS&F project has influenced water quality in the EPA, extending into the ENP, is provided by Florida and Rosendahl (1982), who documented essentially monotonic increases in major ion concentrations in Shark River Slough within the ENP beginning in at least 1959 and extending through 1979. These increases (which approximated a 3-fold increase in major ion concentrations) were attributed to hydrologically connecting Shark River Slough with high ionic strength waters exported from the EAA through the L-67A canal. The evidence suggests that connate seawater and associated contributions of sulfate were not present at current levels observed in the EPA prior to the large-scale modifications to the landscape that began in the late 1800's.

Developing a coupled hydrologic/sulfate mass balance model for the EPA that includes a mechanistic representation of how connate seawater is introduced to the landscape (as recommended by Pollman, 2012) will enable an assessment of whether connate seawater is indeed a modern artifact, or whether it has always been present, and to what magnitude. Such a model would also provide insight with respect to how connate seawater inputs could possibly be reduced.

Flora, M.D. and P.C. Rosendahl. 1982. Historical changes in the conductivity and ionic characteristics of the source water for the Shark River Slough, Everglades National Park, Florida, U.S.A. *Hydrobiologia* 97:249-254.

Miller, W.L. 1988. Description and evaluation of the effects of urban and agricultural developments on the surficial aquifer system, Palm Beach County, Florida. U.S. Geological Survey Water Resources Investigations Report 88-4056.

Pollman, C.D. 2012b. Modeling Sulfate and *Gambusia* Mercury Relationships in the Everglades. Final Report submitted by Aqua Lux Lucis under Contract SP696 to the Florida Department of Environmental Protection, Tallahassee, FL. August 8, 2012. 108 pp.

Comment #18: 886-888: It would seem that a discussion of the ratios of sulfate sources should precede the analysis of changes in excess sulfate. We suggest moving this section and related figures to the beginning of the chapter section.

Response #18: Text will be revised accordingly.

Comment #19: Figure 3B-21: It is a little confusing why both color and dot size vary? Do they somehow reflect different variations of the data?

Response #19: Both dot size (area proportional to *Gambusia* Hg concentration changes) and dot color convey the same information. The two variables are used to make variations in *Gambusia* Hg concentrations across the landscape easier to visualize.

Comment #20: 931-940: As written the chronological order of the sampling and data is a bit confusing. Does the USGS data come before or after 2001?

Response #20: The USGS data comes after 2001. Text will be revised accordingly.

Comment #21: 1004-1007: Not sure that the data in the figure supports the statement that the SUVA254 data shows less variability. Some simple statistics would back up this claim.

Response #21: We will request additional statistical analysis and the text will be revised.

Comment #22: Figure 3B-28 (and supporting text); It seems highly unlikely that radial oxygen loss influences on pore water chemistry would show variation of the scale measured. Several studies in wetland environments do show radial oxygen variation, but at a spatial magnitude one, or two times smaller. Differences on the scale of mm would likely be more revealing, but admittedly hard to do in the field.

Response #22: For clarification, lines 1089-1091 will be revised in the final report as follows: "Indeed, porewater monitoring data of this study indicate that sulfide levels tend to decline in close proximity to the plant stems (Figure 3B-28). This suggests that an oxidized rhizosphere, perhaps due to a greater density of fine roots in soils near the stems, may be inhibiting sulfate reduction or oxidizing sulfide."

Comment #23: 1124-1136: Are there significant areas of leafy greens grown in the EAA? If not, it is not clear how this paragraph relates to the bigger picture.

Response #23: There are approximately 10,000-12,000 acres of leafy greens grown in the EAA. Most of this land grows 2-3 crop cycles per year, with higher inputs, such as fertilizer, compared to sugarcane. We will reconsider the relative importance of leafy green crops compared to the dominant land use of sugarcane farming (450,000 acres), and edit the Chapter text as appropriate.

Comment #24: 233: Watershed area??

Response #24: The text reads: "These results, which are unweighted for area, indicate that the distribution of fish tissue Hg concentrations in the Everglades is higher compared to that observed in Florida streams and rivers external to the Everglades and substantially higher than concentrations found in Florida lakes." We would have preferred to weight water bodies by area to calculate mean and median THg in largemouth bass. For example, for the Everglades, median THg would ideally be weighted by the areas of WCAs 1, 2, 3 and the ENP. We do not however have the sample distribution necessary to do this, particularly in the ENP.

Comment #25: 311: *Should this be Figure 3B-6?*

Response #25: The text will be corrected.

Comment #26: 527 *spatially and temporally*

Response #26: You are correct that this should read “spatially and temporally” and we will correct this.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 4

William Baker, Jonathan Madden and
Pamela Wade with coauthor contributions

Level of Panel Review: Accountability

Reviewer: V. Novotny (AA)

Posted: 10/9/12 @ 5:03 PM EST

Introductory Comments and Responses

Comment #1: Chapter 4 presents an update on the nonpoint source programs mandated by the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA). These program, as stated in the draft chapter, address the reduction of phosphorus and other pollutant loads through on – site measures that reduce or prevent pollution at its source. Most of the surface pollution to the Everglades originates from nonpoint sources. However, atmospheric deposition, not addressed in this chapter, is greater than the surface NP sources. The chapter outlines the programs in Lake Okeechobee, St. Lucie River, Caloosahatchee River, C-139, C-111, Everglades Agricultural Area (EAA), and some smaller watersheds

Response #1: No response is requested by the reviewer. The authors agree that this chapter does not address atmospheric deposition.

Comment #2: Apparently, the detailed review of the NP programs is performed every three years and WY 2011 (last year) was the off-year for such review. Hence the Chapter 4 of the 2013 SFER is somewhat limited in substance and results. Furthermore, Chapter 3A revealed that most phosphorus, a key pollutant causing the water quality (eutrophication) problems in the protected Everglades area, originates from difficult to control atmospheric sources. Apparently, the structural measures discussed in the chapter may have only limited or no impact on reduction of atmospheric sources. On the other hand, atmospheric P sources are regional and the only source is wind soil (or mine spoil) erosion. This reviewer recently noted that a large Bone Valley phosphorus surface mine, the only one in the US, is at the edge of the Everglades and Lake Okeechobee systems and could be most likely a source of atmospheric P, more than agricultural and urban sources reported in the chapter combined. There is no doubt that surface mines with heavy excavation machineries and no surface protection are a major source of atmospheric phosphorus. It is expected that the mining will run out of phosphate ore in about 40 years. So far no one looked yet at this possibility and the mine impact on the Everglades system is unknown. The reviewer encountered the same situation thirty years ago when dealing with sources of nitrogen limited and heavily impacted 500 km² Lagoon of Venice in Italy, where a nitrogen fertilizer manufacturing plant was located right on the shores of lagoon. At this time the effect of the Bone Valley mine on P inputs to the Everglades has most likely never been assessed.

Response #2: No response is requested by the reviewer.

Comment #3: The chapter also revealed that plans for NP controls have been mostly developed but they are still in the first phases of implementation. Nevertheless, the chapter reported a 71% (154 metric tons) reduction of the total phosphorus (TP) when compared with the predicted load. It is not clear what is meant by the “predicted” load. WY 2012 was hydraulically and climatologically an average year and in the 2011 and 2012 (calendar) years, droughts did not impact Florida.

Response #3: Source control programs within the Southern Everglades mandated areas are fully implemented. In the Northern Everglades source control programs are under development in the Caloosahatchee and St. Lucie Rivers and Estuaries, and under partial implementation in the Lake Okeechobee watershed.

The predicted load is the estimated load that would have been discharged from the EAA during the pre-BMP base period given the current year rainfall amount and monthly distribution. The reported WY2012 reduction in TP load of 71% was solely for the Everglades Agricultural Area Basin, not the Southern Everglades or entire source control program.

Chapter 2 of the 2013 SFER provides a detailed discussion on the meteorology of WY2012.

Comment #4: *The goal of the NP control plans is to minimize the undesirable flows to the estuaries and improve the quality of water delivered to Lake Okeechobee and the estuaries through source control programs, construction research projects, and water quality monitoring.*

Response #4: No response is requested by the reviewer.

Comment #5: *For the Southern Everglades, the source control program planning was incorporated into the Long-Term Plan for Achieving Water Quality Goals in the Everglades Protection Area (see Chapter 5). The strategy includes implementation of BMPs for phosphorus reduction, (2) regulatory programs, (3) voluntary programs, (4) educational programs, and (5) integration with local and regional water quality projects.*

Response #5: No response is requested by the reviewer.

Comment #6: *Apparently, because WY 2012 is the off year of the three year assessment cycle, the first twenty pages of the chapter describes the programs in a sequence of 1 to 3 pages status reports but does not provide results or technical details. Hence, in this review the programs were noted with a reference to the last year report and reviews which described the intermediate results.*

Response #6: The Lake Okeechobee, Caloosahatchee River and St. Lucie River protection plans are on a three year cycle with the last updates occurring in 2011 and 2012 respectively. Updates for the EAA, C-139 and non-ECP basins occur annually and are included in the chapter.

Comment #7: *A general and repeating comment by this reviewer pertains to the use of units. The chapter is not consistent with units, it uses US and SI units in the same table and added units the authors invented. Repeatedly, it has been emphasized that the SFERs are read not only by US readers but has and should have an international audience. Furthermore, today scientific reports written by government agencies are consistent with units and most predominantly use metric units. Hardly anyone today uses ppm for mg/L. Some problems will be identified; however, in most cases the authors provide conversion factors.*

Response #7: The choice of units considers the regional stakeholder audience as well as the potential international audience. The introduction to the final SFER will contain a unit conversion table. Conversions will also be added to the chapter where applicable.

Specific Comments and Responses

Comment #8: *Page 4-4 – Table 4-1. This table summarizes the total (in metric ton) and specific (in pounds per acre). It identifies West Caloosahatchee and Lower Kissimmee watersheds with the highest phosphorus loads but apparently these loads may not be directly affecting the Everglades. The Kissimmee River is the main tributary of Lake Okeechobee. Apparently, only a small portion of the Kissimmee loads, after attenuation in the lake, are discharged into the Everglades but not at all times. The Caloosahatchee River discharging into the Gulf of Mexico and St. Lucie River discharging into Atlantic Ocean are the main outlets from the Lake Okeechobee.*

Response #8: The District source control programs which began to reduce nutrients that ultimately entered the Everglades Protection Area have been expanded by the Northern Everglades and Estuaries Program (NEEPP [Section 373.4595, Florida Statutes] to include source control requirements for Lake Okeechobee, the Caloosahatchee River and Estuary and the St. Lucie River and Estuary. In the NEEPP the legislature found that water quality improvement of these areas is essential to the protection of the greater Everglades ecosystem. The evaluation of nutrient loadings in these areas will be necessary for future assessments of source control progress.

Comment #9: *Page 4-7 - lines 159-160. The chapter identifies voluntary and regulatory approaches used to control the nonpoint sources. The authors state that the regulatory source control programs have been demonstrated as the foundation for effective strategies for reducing pollution loads. The program is still in its initial phases of implementation, some results are not promising. Maybe this statement expresses premature optimism.*

Response #9: Depending on the specific watershed, source control programs in the Northern Everglades are in development or in the initial phases of implementation. The source control program in the EAA has been in place for 17 years averaging a 55% load reduction and preventing 2,565 metric tons of phosphorus from leaving the EAA.

Comment #10: *Page 4-8 – Table 4.2 lists 9 nonpoint pollution control programs and 4 point source controls in the Northern Everglades. The NEEPP covers a phased, comprehensive protection program that included construction projects, source control programs, and research and water quality monitoring programs. The source control programs are carried out by the SFWMD, Florida Department of Environmental Protection (FDEP), and Florida Department of Agriculture and Consumer Services. The University of Florida is responsible for the development of effective yard fertilizer controls. The pollutants of concern are phosphorus for Lake Okeechobee and both phosphorus and nitrogen for the estuaries of the two rivers. Programs with the largest impact could be the programs of the Florida Agriculture and Consumer which are implementing Agricultural BMPs, animal manure applications and urban turf fertilizer rules.*

Response #10: No response is requested by the reviewer.

Comment #11: *Pages 4-13 to 4-20. These pages describe the status of the control programs in the Northern Everglades Watersheds. The programs are both voluntary and regulatory. The regulatory programs rely on implementation of Environmental Resources Permits that apply mostly to control of pollution by stormwater runoff and wetland dredging and filling wetlands. Voluntary approaches are mostly for agricultural sources.*

Response #11: The Northern Everglades and Estuaries Protection Program (373.4595 Florida Statutes) mandates a multifaceted approach to reducing nutrients by improving the management of sources within the watersheds through existing regulations (including the District's source control rule Chapter 40E-61) and through the development of the BMPs. The NEEPP requires agricultural operations in the Northern Everglades to implement BMPs or demonstrate compliance with state water quality standards through water quality monitoring.

Comment #12: *Page 4-14 Table 4-3. This table lists the percentages of acres with permits. Some percentages seem confusing since the permitted total acreages for most watersheds are greater than 100%. This should be explained. Do some basins have two permits? What does it mean that basins are located in overlapping watersheds? Watersheds are separated by watershed divides and do not overlap. Provide a conversion from acres to hectares. Linking Table 4-3 to 4-1 is not clear, the majority of programs listed in Table 3 are located in the Caloosahatchee and St. Lucie watersheds which do not discharge into the Everglades.*

Response #12: An area within a basin may be covered by both an Environmental Resource or Surface water permit and a Works of the District permit. This is shown visually in Appendix 4-1 and 4-4. A note will be added to the table in the final chapter explaining that the percentages are not mutually exclusive. The hydrology in the South Florida Water Management District is largely

controlled by water control structures operated by the District or the Army Corps of Engineers. In some cases water can flow to multiple watersheds.

A conversion factor from acres to hectares will be added as a note to the table in the final chapter.

The South Florida Water Management District source control programs consider nutrients that discharge to Lake Okeechobee, the Caloosahatchee River and Estuary and the St. Lucie River and Estuary as well as the Everglades Protection Area.

Comment #13: *Pages 4-15 to 4-17 briefly describe the WY 2012 and WY 2013 anticipated activities. They include performance measures development, nutrient monitoring, and implementation of BMP regulatory programs. These programs are ongoing and very little specific results were provided in this section.*

Response #13: The development of performance measures in the Northern Everglades is ongoing and it is expected that details will be presented in future chapters. Nutrient monitoring results for WY2012 are presented in Table 4-1 and detailed annual results are presented in Appendices 4-1 and 4-4.

Comment #14: *Table 4-4 lists the programs administered by the Florida Department of Environmental Protection. Again no specific results were provided in the section.*

Response #14: The placement of the table in the chapter implies that the programs are administered by the Florida Department of Environmental Protection (FDEP); however Table 4-4 presents data on the Florida Department of Agricultural and Consumer Services BMP enrollment. The placement of the table will be modified in the final chapter to better reflect the responsible agency. Refer to the SFER 2012 Chapter 4 for a detailed discussion of the FDEP programs.

Comment #15: *Pages 4-21 to 4-38 focus on the source controls by the Everglades Construction Projects (ECP) in the EAA area and C-139 basin but the bulk of writing is about the C-139 basin. Based on Table 4-1, the P load from the EAA is 63 metric tons and the unit load is 0.32 kg/ha (0.29 lbs/acre), those of C-139 basin are 15 metric tons and 0.22 kg/ha (0.2 lbs/acre) respectively.*

The EAA basin is required to achieve a 25 % reduction of the loads when compared to the baseline period. The C-139 basin is required to meet and maintain phosphorus levels relative to the EPA defined baseline period.

Response #15: No response is requested by the reviewer.

Comment #16: *Page 4-22 – lines 507 - 508 report the P load decrease of 71 percent, when compared to the predicted load from the pre-BMP baseline period. This represents the seventh year the basin is in compliance. These results are also listed in Table 4-5. In the Table ppb should be changed $\mu\text{g/L}$. The average listed concentration of the flow from EAA area of 111 $\mu\text{g/L}$ is still not acceptable.*

Response #16: The use of ppb as the water quality unit is consistent with previous years SFER source control reporting, the Everglades Forever Act which mandates the EAA source control program and recent STA operational permits issued by the Florida Department of Environmental Protection. A reference to the use of ppb in this context will be in the final SFER introductory unit conversion table. Prior to entering the Everglades Protection Area (EPA) water quality is further improved by Stormwater Treatment Areas (see Chapter 5 of the 2013 SFER). Note that the EAA achieved its seventeenth year of compliance, not seventh, in WY2012.

Comment #17: *The trend and target values of the percent P load reduction from the EAA area are shown on Figure 4-8. The confusing issue in this figure is the fact that the target was met as early as 1992 when no BMPs or STAs were implemented. Yet the P concentration in Lake Okeechobee and canal inlets into ENP remain high. Is the target too lenient, giving a false satisfaction to the agency?*

Response #17: To account for inherent statistical uncertainty in the target regression model, noncompliance with the target is measured as a three consecutive year test. Alternately stated, three consecutive years of meeting the 25% target provides certainty that the goals are being met. This did not occur until WY1997 after full implementation of BMPs. The 25% target is mandated by legislation.

Comment #18: *Table 4-6 on page 4-25. This table is a units mismatch nightmare. Provide a conversion or convert (easily in Excel) inch to centimeter (cm) or millimeters (mm). Substitute $\mu\text{g/L}$ for ppb which nobody is using today. There is no conversion between the two. Kac-ft is an invented mismatch of units. You cannot put together Kilo which is a metric prefix with acre-ft which is an archaic US unit. A proper metric unit for this magnitude of flow would be 106 m^3 and a proper US unit designation would 103 acre-ft. Conversion between the two is $1000 \text{ acre-ft} = 1.233 \times 106 \text{ m}^3$.*

Response #18: The authors recognize the mix of units in the table. The meteorological standard for measuring rainfall at the SFWMD is inches. A conversion factor to cm will be provided in the final SFER introduction. The use of acre-ft for large volumes of water will also be described in the SFER introduction. The use of K as an abbreviation for thousand has been used by other authors in this and other contexts, but since it may not be standard, the table heading will be changed to 103 acre-ft in the final chapter. The use of ppb in the context of this chapter is described above.

Comment #19: *Page 4-28. Because the BMPs in the EAA are mostly voluntary, the list of activities describe briefly research, workshops, and development of regulatory programs.*

Response #19: BMPs in the EAA are mandated by the Everglades Forever Act [Section 373.4592(4)(f), F.S.] and permits issued in the EAA by the SFWMD under Chapter 40E-63, F.A.C. have specific BMP implementation requirements. A discussion of the EAA Basin source control strategy including mandatory BMPs starts on page 4-26 of the draft chapter.

Comment #20: *Pages 4-29 to 4-37 focus on C-139 basin. Table 4-7 presents the results of observed load with the base line calculations. The basin load apparently meets the guidance limit. In the table again use $\mu\text{g/L}$ instead of ppb without a conversion. The five year TP concentration from the basin is high, compared to the limit of $10 \mu\text{g/L}$ to be achieved in EPA, in spite of meeting the load limit.*

Response #20: The use of ppb as the water quality unit is consistent with previous years SFER source control reporting, the Everglades Forever Act which mandates the C-139 source control program and recent STA operational permits issued by the Florida Department of Environmental Protection. A reference to the use of ppb in this context will be in the final SFER introductory unit conversion table. Prior to entering the Everglades Protection Area (EPA) water quality is further improved by Stormwater Treatment Areas (see Chapter 5 of the 2013 SFER).

Comment #21: *Table 4-8 on page 4-32 has the same unit mismatch problem as Table 4-6*

Response #21: See response on the Table 4-6 comment above.

Comment #22: *Table 4-9 describes BMP levels and performance but does not specify what the "levels" are. The table is confusing.*

Response #22: The levels referenced in the table refer to practices required by the C-139 BMP permits. For example, a permittee may have been required to add additional sediment controls as the basin moved from Level I to Level II. An explanatory note will be added to the table.

Comment #23: *Figure 4-10 – page 4-34. Change ppb to $\mu\text{g/L}$ on the Y axis label, if possible. The figure shows very high concentrations before 2010 followed by a sudden drop in HY 2010 year. Can it be explained?*

Response #23: Please see response to Comment 20 above on the use of ppb in the context of this chapter. A discussion of concentration trends can be found in the 2012 Appendix 4-2 and draft

2013 Appendix 4-2. In part, climate and the gradual effect of BMPs are likely impacting basin discharge concentrations.

Comment #24: Page 4-35, line 790. Table 4-1 listed the C-139 watershed area as 168,450 acres. Reconcile the two values.

Response #24: The authors appreciate the identification of the difference. The permitted area is close to the watershed area. The discrepancy will be reconciled in the final chapter.

Comment #25: Page 4-36, lines 861-867 reports a disappointing low performance of aboveground impoundments for reducing phosphorus. This follows the last year results of more detailed reporting on wetlands and ponds removal efficiencies which were also disappointing (see last year review of Chapter 4).

Response #25: Impoundments vary considerably in shape, outfall structure(s) location, topography, vegetation, soil, etc. which all impact the impoundment treatment efficiency. In addition, historical loads, current in flow loads, and operation, may also affect the impoundment treatment efficiency. The efficiency results from this study are site-specific and can only be applied to this impoundment, but the identification of physical factors likely affecting efficiency can be applied to future impoundment designs.

Comment #26: Page 4-38 – line 932 -934. HY 2012 was not a drought year in Florida. See this reviewer's critique of Chapter 3A which presented the extent of calendar years 2011 and 2012 severe and extensive droughts. Florida was not affected.

Response #26: Chapter 2 of the 2013 SFER provides a detailed discussion on the meteorology of WY2012.

Comment #27: Pages 4-39 to 4-44 describe the status of source controls in the non -ECP basins. There are seven small basins outside of EAA and C-139 basins with relatively small contribution to the EPA.

Response #27: No response is requested by the reviewer.

Chapter 4 Review Closing Comments and Responses

Comment #28: WY 2012 is an off-year for evaluation and synthesis of the nonpoint nutrient source programs. The chapter consists generally of a number of short progress reports briefly describing activities, ongoing and planned, but provides very little specifics. This makes the chapter confusing but one can assume that this chapter at the board review may receive less attention. This makes the chapter confusing. If possible, this chapter should provide the highlights of the last full assessment, summary of corrective actions that transpired from the last assessment, and then continue with the description of current and planned activities.

Response #28: The authors will include current water year as well as prior water year performance assessments for the Northern Everglades in future SFER Chapters once performance measure development is complete. Any changes from the protection plan updates will be noted; otherwise those documents are the best source of current information. In the Southern Everglades, tracking of prior year assessments is included in tables and graphs.

Comment #29: This chapter has a problem with the consistency of units, mixes both metric and US units.

Response #29: The choice of units considers the regional stakeholder audience as well as the potential international audience. The introduction to the final SFER will contain a unit conversion table. Conversions will also be added to the chapter where applicable.

Comment #30: Similarly to the last year evaluation, most of the source programs are in the implementation phases. The district is still rapidly working to implement the BMPs to achieve the objectives of the river and lake protection plans

Response #30: Details on the status of implementation are described in the responses to specific comments above.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 5

Delia Ivanoff and Chapter Coauthors

Level of Panel Review: Technical

Reviewers: O. Stein (AA); P. Dillon (A)

Posted: 10/11/12 @ 8:59 AM EST

The review is divided into several sections. First are several broad questions and comments regarding the interpretation of reported results followed by relatively minor questions about specific sections, sentences, phrases, etc. This is followed by comments on figures and tables, editorial suggestions and lastly summary and recommendation comments.

Broad questions and comments that should be addressed:

This chapter is straightforward, very clearly written and easy to read. Much of the work is very descriptive, which is fine, but there are a few subjects that could be addressed in a more in-depth fashion, e.g. the importance of droughts.

***Response #Broad Question (BQ)1:** The authors will provide further discussion on some of the topics, including the importance of droughts. Specific to drought effects, the chapter contained the information the authors and management believe is necessary to describe the events in WY2012, including the impacts of drought to individual STAs and the steps taken to minimize drought impacts.*

As noted last year, the new more streamlined format of the chapter, starting with overviews of each STA followed by brief reports on selected ongoing research project and very brief overviews of recreational opportunities and the Long Term Plan, is a considerable improvement over previous formats. The reintroduction of brief overviews of the research programs, as recommended in last year's review, is a welcome addition compared to last year. While improvement compared to last year is noteworthy, a still-valid criticism is that there is, in general, a dearth of interpretation of the reported data. The second question above asks whether there are *other* interpretations of the data, but too often the document simply reports data rather than any scientific analysis of the data. It is difficult to offer an alternative interpretation when an original is not provided. This criticism is leveled more on the performance reporting section than on the research summaries, where there is typically at least a paragraph of discussion and/or recommendations but these too are all very descriptive; some provide useful information but rarely is it put in context of what is already known about the topic from other work.

***Response #BQ2:** The authors continue to improve the chapter in response to previous peer review panel comments. This chapter is intended to be an annual STA report, summarizing the annual STA performance, an evaluation of conditions relevant to STA performance, facility status, operational challenges, and enhancements during Water Year 2012. As in previous years, the best available information collected during Water Year 2012 was provided in the draft chapter. Most data were gathered from non-experimental research and are not manipulated by the researcher. While the authors believe that comprehensive analysis and interpretations of the data are necessary to enhance understanding of the STAs and continue with this effort, reporting this annually has not been possible, particularly because of a contrasting goal of streamlining and keeping the chapter concise. In 2012 SFER, for example, correlation analyses between period of record inflow TP concentration/loading and outflow concentration were included. While the District scientists continue with this type of analyses, we acknowledge the difficulty of*

being more specific on cause and effect, due to large variability in the data sets and the inherent variabilities among the STAs and among cells within each STA. The findings will be included in next year's SFER. Details about data analysis approach and new findings will be included in the final chapter.

The authors agree that better analyses as to cause and effect could come from controlled studies, however, that will require large research facilities and tremendous amount of resources. Significant resources have been spent in earlier years to evaluate different treatment technologies and those evaluations have been published in earlier SFERs. The District is currently developing a science plan that will aim to address information gap that could lead toward improvement of STA performance and a better understanding of the biogeochemical factors and mechanisms that control ultra low phosphorus concentrations in these large treatment wetlands.

The panel appreciates that the authors reorganized the performance section first by STA then by parameter of interest as was suggested last year. Hopefully this format will allow the District focus more on better interpretation of results leading to better management of individual STAs and the entire STA infrastructure as a whole.

Response #BQ3: *The authors continue to improve the chapter, based on the peer review panel's suggestions.*

It is not clear why the PSTA cells in STA1E have been abandoned while the PSTA cells in STA 3/4 are still in an active research mode. I understand that the cells in STA1E were initially developed by the Corps but considering the expense to create them and the obvious scientific merit of replication, it seems odd that the cells in STA1E would simply be abandoned. Please explain the logic behind this decision.

Response #BQ4: *An explanation will be provided in the final chapter. In order for STA-1E to achieve its authorized purpose and meet performance standards, decommissioning of the Periphyton Stormwater Treatment Area (PSTA) Demonstration Project is needed. This is due to the fact that the PSTA Project and its associated structures restrict the volume of water that can be treated by STA-1E and limit the flow capacity of the eastern flow-way of STA-1E. The PSTA Project was initially planned to be complete by December 2007, however, due to delays and extensions, the project ended in December 2010, with only 12 months of usable data. In weighing the incremental benefits of additional data that could be obtained from resumption of the project, the South Florida Water Management District's support for PSTA Project removal, the lack of Federal or state agency support for continuance of the PSTA Project, and after coordinating with the public, the U.S. Army Corps of Engineers decided against the resumption of PSTA Project testing and is proceeding with decommissioning of the PSTA Project in STA-1E. At this time, the potential to improve the reduced operational capacity of STA-1E is more desirable than any potential benefits gained from the continuation of the PSTA Project (Source: USACE, 2012. Stormwater Treatment Area 1 East, Palm Beach County, Florida, Environmental Assessment and Finding of No Significant Impact, Decommission of Field Scale Periphyton Stormwater Treatment Area Demonstration Project, U.S. Army Corps of Engineers, Jacksonville District, Jacksonville, FL.)*

As the panel may have read in the draft chapter, the District is continuing its PSTA research efforts on the STA-3/4 PSTA facility. One advantage of this facility is the ability to operate it in a manner that does not restrict full use of the central flow-way of STA-3/4.

With the build out of Compartment B and Compartment C nearly completed the capacity of the STA system will be increased significantly. This has to be considered a positive development as more water can be treated but it is clear that management of the STA under drought conditions has been problematic with the current area in STA. How will the District keep even more area wet during the dry season, especially in drought years considering the importance of keeping cells hydrated to prevent P concentration spikes upon rewetting? (See also the comment on line 798)

Response #BQ5: *Since 2008, the District has been implementing a STA Drought Contingency Plan to minimize drought and dry season impacts. For example, supplemental water from Lake Okeechobee is delivered to most of the STAs, depending on water availability. As part of the overall Everglades Restoration Strategies, Flow Equalization Basins (FEBs) will be built. These areas will add to the potential supplemental water source for the STAs during dry seasons. The combination of FEBs and STA expansion will provide greater ability to hold water longer at the onset of the wet season without discharging from flow-ways that have dried out. This will allow for recovery of the phosphorus treatment mechanisms before discharging from affected flow-ways. The revised chapter will include these and more details related to STA management during dry conditions.*

Considerable effort is put toward identifying the species of SAV in the various cells (it is a component of all STA sections and even some continuing research). A few years ago an explanation of which species are considered more and less desirable was presented, but this information has not been relayed to the reader for several years. In line with the comment that more interpretation is required, it would be good to include a brief discussion of which species are (at least at this point in time) considered desirable and undesirable.

Response #BQ6: *The authors will include a brief discussion about this topic in the final chapter.*

Drought clearly causes re-release of phosphorus and is a critical factor in the P removal efficiency of these systems. This is addressed often in the report but the potential mechanisms of P release are never really discussed in any detail nor are there attempts to determine the mechanism in the experimental section. There may be supporting sediment data in other reports, appendices, etc. but we would like to see either a brief summary of evidence supporting potential release mechanisms or direction to where the appropriate data are. For example, it is well-known that in lakes, oxic conditions in bottom waters and surface sediments favor the presence of oxidized iron (Fe^{3+}) which forms quite insoluble complexes with phosphorus minimizing P release during droughts, while reducing or anoxic conditions favour the much more soluble reduced Fe and release of P from sediments. Thus, drought might logically be expected to lead to oxidation of sediments/soils and trapping of more phosphorus. On the other hand, oxidation of organic matter would be more rapid under drought conditions and oxidation/decay of organic matter may release associated P and other nutrients. In addition, high calcium levels may result in Ca control of the P cycle with Fe being less important. In summary, we would like to see some discussion of why drought causes P spikes in these systems. This is particularly important because of the potential for increasing frequency of droughts as a consequence of climate change. It is not clear to me that there is any long-term plan developed or being developed to address this likelihood and its consequences for these P removal techniques.

Response #BQ7: *As mentioned in an earlier response, the authors will include further discussion on drought impacts, including a discussion of why drought causes P spikes in these systems.*

The overview section should focus on fluxes rather than concentrations because it's not concentrations that matter. Perhaps this is done because regulations are written as allowable or target concentrations rather than load reductions?

Response #BQ8: *The focus of the chapter is to document the status of the STA systems as they pertain to the water year performance. Performance is reported in terms of both concentrations and load reductions, in accordance with permit conditions for the STAs.*

Throughout the overview section- a little more detail about sampling frequency, storm events etc. in here would be nice; there are no standard deviation estimates or confidence limits given for any of the data in the section; if this is available in appendices or other volumes it would be good to refer to it here.

Response #BQ9: *The authors will add information on sampling frequency, storm events, etc. to the final chapter. In the overview section, data on the system or individual STAs are yearly or multiple-year total so there are no standard deviation estimates for those values. For multiple-*

year mean data such as inflow and outflow TP concentrations in Table 5-1, the authors will add SD or CL for the mean values.

Specific comments questions by line number.

32 and elsewhere: there are too many significant figures used frequently; 3 significant figures is probably realistic, not 6

Response Line #32: *The plan is to keep the numbers as they are for the 2013 SFER, but make the suggested changes for next year when reporting will be based on new permits.*

33 and elsewhere: the mix of acre-feet and metric tons is strange; surely the hydrology can be expressed in metric units too

Response Line #33: *Same as response #32.*

257: Figure 5.7 is mentioned here and again much later in the document, yet there is no Figure 5.7 in the document. It looks like it was simply omitted.

Response Line #257: *The figure numbering will be adjusted accordingly. The references to Figure 5.7 actually pertains to Figure 5.4 the figure which depicts the online/offline status of STA flow-way). This figure was moved earlier in the document for a better placement, hence the change in figure number.*

280: The a long-term plan will need to address how to deal with increasing drought frequencies
Response Line #280: *See response to BQ#5.*

540, 654, 678: It is not clear what land use was converted to what new land use.

Response Line #540, 654, & 678: *There was no land use change; the statements discussed vegetation conversion of the southern portion of STA-2 Cell 2. The authors will revise the text and specify that the conversion was from cattail (emergent vegetation) community to submerged aquatic vegetation community.*

555-556: Here and in a few subsequent section authors make claim that previous land use has an influence on performance. However no explanation as to why the District believes performance of areas that were previously farmed are not as effective as areas that were in a more native state. Some background to this statement is warranted.

Response Line # 555-556: *The authors will add statements clarifying the relevance of antecedent land use in the STAs. Briefly, we have been hypothesizing, based on published literature on phosphorus cycling in wetlands, that the previously unfarmed natural area has more stable pools of phosphorus and the farmed areas have more labile phosphorus storage. Flux of phosphorus from the soil to the water column is affected by the stability and transformations of phosphorus in the underlying soil column. Future research in the STAs will likely include further investigations on the characteristics of and mechanisms within STA-2 Cell 1 that result in consistently very low outflow TP concentrations.*

557-563: In general it seems to be that the preferred flow path is to have water flow first through an EAV cell, then through an SAV cell (I am assuming this is a prefer BMP). Most data seems to support that configuration as increasing performance. Yet in this case flow through an EAV cell only consistently produces the lowest P concentrations. Can the District offer any possible explanations?

Response Line #557-563: *The authors believe that the excellent performance of Cell 1 of STA-2 is due to a combination of factors, including the fact that the area was never farmed (see Response to Lines 555-556), moderate hydraulic and P loading rates, low inflow TP, good vegetation coverage (cattail and sawgrass), and strategic water management to minimize occurrence of dryout (despite known seepage losses). Due to the fact that there are many variables involved, it has been challenging to point to a single factor. Future research in the*

STAs will likely include further investigations on the characteristics of and mechanisms within STA-2 Cell 1 that result in consistently very low outflow TP concentrations.

678: Here it is suggested that the land use change in southern portion STA2 cell 2 is from EAV to SAV. This change is not reflected in any of the figures (including Fig 3 of Appendix 5-1).

Response Line #678: *The authors will revise the figures and/or figure captions to clarify that the southern portion of Cell 2 was converted from EAV to SAV.*

798: It would appear that a way to keep SAV cells hydrated and let EAV cells dry out (at least occasionally) would be add structures or reconfigure flow to put water directly into the SAV cells without passing through the EAV cells first. It appears this might be happening in some cases, will these improvements be continued?

Response Line #798: *Yes, this is one of the drought contingency practices in the STAs, to conserve water and deliver them to the priority cells (SAV cells). District scientists and engineers are continuing to look at improvements in other cells to allow direct delivery of water, by gravity or pumps, to SAV cells.*

842: Looks like the date is wrong here as Fig 5-27 shows a depth of approximately 2 ft. at this time.

Response Line #842: *This was an error; the correct date is June 16, 2012. The figure caption will be changed with the correct information.*

871: Was the water level too low for the boat?

Response Line #871: *Yes, the water level was too low and the cattail vegetation was very dense for safe airboat access in Cell 1B.*

882: The entire section focused on STA 5/6 is not as clear as the previous sections. In numerous locations components mentioned in the text (such as flow-ways, cells etc. are either not identified in any figures or are sometimes inconsistent (north flow-way versus Flow-way 1 for example). It is nearly impossible to follow this section if the reader cannot identify which geographic sections the text refers too. Similarly Compartments B and C are often mentioned but identified in the figures for the appropriate STAs.

Response Line #882: *The authors will edit this section to clarify and keep cell and flow-way nomenclatures clear and consistent.*

967, 975, 976, 978: It appears that some of the figure numbers are incorrect, but it is hard to tell which are off. Clearly the text does not match the referenced figure

Response Line #967, 975, 976, & 978: *The authors will make the corrections in the final chapter. Some of the figures were moved and we failed to update the references in the text.*

979, 1077: What is an “environmentally sensitive area”?

Response Line #966 & 1077: *These are cultural resource areas within the cells that must be preserved and kept from inundation. Due to the sensitivity of this issue for the parties involved, it is preferred not to include too much detail in the chapter.*

966: Since there is more than one cell labeled 2,3,4, and 5, the STA number needs to be included in all nomenclature (See comment on line 882)

Response Line #966: *The authors will edit this section to keep cell nomenclatures clear and consistent.*

1008-1012: With the exception of STA5 C1A, it is hard to see any obvious trend in the data. My guess is that there is some threshold dryout period that must be crossed before a real effect is apparent, as perhaps seen in STA5-C2A. Rather than taking an arbitrary concentration level (50

ppb), why not use a relative concentration for before and after drying as a threshold for a TP spike. Such a criterion might provide more data to analyze over the POR.

Response Line #1008: *The authors will be revising the analysis to include concentration spikes down to 20% of pre-dryout TP concentrations and including more data.*

1033 – it would seem to me that getting permits before building the system would make sense – why not?

Response Line #1033: *The permits referred throughout the chapter are operating permits. Construction permits for Compartments B and C were issued prior to the start of construction. The operating permits were issued after the facilities were built.*

1055: Though there is some detail in earlier reports about the herbicides used and toxicology issues, but it would be useful to at least identify what is being used

Response Line #1055: *The authors will include some details about the herbicides being used in the final chapter; this information is also included in Volume III, Appendix 3-1.*

1200: The rationale for this experiment is not clear. Why should periphyton matter? Its biomass or potential biomass couldn't be great enough to trap significant P could it? Some calculations showing its relevance to support the experiment would be useful.

Response Line #1200: *The authors will include statements in the final chapter to clarify the rationale for the study. A PSTA-based treatment system relies on periphyton that grows in shallow aquatic environments, and that has been shown to be able to reduce phosphorus in surface water to very low concentrations. Mesocosm and field-scale studies related to the PSTA technology have been conducted since 1999; the STA-3/4 PSTA Project is the largest in spatial scale and has been operated for the longest time. Data collected to date in the STA-3/4 PSTA Project has been promising, however various issues with the hydraulic data made it difficult to interpret the performance data. For this reason, for the next three water years (WY2013-WY2015) a PSTA Research Plan with intensive scientific investigations is being implemented to provide more accurate assessment of PSTA technology performance. The starting P concentration is quite low and is primarily in organic or particulate form; therefore, the total P to be removed to meet the target concentrations is small. The periphyton has the capacity to remove that remaining P from the water column.*

1274-1277: The meaning of these statements is not clear; perhaps a rewording is in order.

Response Line #1274-1277: *In contrast to previous reporting, the surface-water aerial phosphorus loading rates for the PSTA cell's POR data are not adjusted to represent an assumed scenario that the flow-way was online for the whole water year.*

1327-1331: A more detailed plan figure would help with interpretation of these results.

Response Line #1327-1331: *The authors will include a map that illustrates where the PSTA water quality transects are located.*

1371-1467: While the study is obviously still continuing and the issue may be due to start-up effects, it is a little disheartening that the mesocosm studies results seem to be running counter to what has been observed in the full scale cells. One could argue that the anomalous results basically show that mesocosms often don't work because setting them up disturbs the system they are trying to represent too severely. If these trends continue, the study will highlight more questions than answers.

Response Line #1371-1467: *In an effort to be concise, the principal investigator did not include all the details about the study rationales and initial findings in the draft chapter. Details will be added in the revised chapter to clarify the purpose and interpretation of start-up findings. Briefly, the study was designed to employ the STA-1W outflow as its inflow. Consequently, the study has been operating under a very low-phosphorus (P) condition (24.1 ± 6.1 ppb) that was much lower*

than the inflow TP of full-scale STAs. Also, for newly established constructed wetlands, there have been instances during the start-up/stabilization phase when outflow TP was greater than inflow TP. For this mesocosm study, the greater outflow TP was largely due to P release from soil, which was suggested in literature and observed in the field-scale STAs. The soil used for the mesocosms was previously enriched STA soil, which contain labile P. Thus, the principal investigators feel that the initial results during the first two years was not abnormal; instead they typify P dynamics in newly constructed wetlands under a low inflow TP condition. It is hypothesized that the magnitude and period of P release would be different among vegetation types depending on their uptake, decomposition, and associated P removal mechanisms. The information will provide insights into wetland restoration and management strategy. Finally, the present proof-of-concept study expects that certain vegetation types (such as water lily and sawgrass) would exhibit a better P removal performance than other treatments (including cattail and SAVs).

1415: to comment on the mesocosm experiments in detail, I think

Response Line #1415: *The authors are not clear about this comment and are not able to provide further comment on this. However, the authors would like to make a note that the mesocosm study is ongoing and plan on presenting a more comprehensive report on next year's SFER.*

1468-1543: It is hard to justify the conclusions that are drawn from the data. There appears to be little to no correlation between density and dry out (see Fig. 5-51 versus Fig. 5-50). There is no change in density due to the dry-out of Feb 2011-June 2011. This study needs to be either conducted under more controlled conditions to test cause and effect or be abandoned.

Response Line #1468-1543: *The authors agree that studies with more controlled conditions can result in more specific cause and effect findings. However, this particular section was done to evaluate the success of water level drawdown as a management strategy in an area previously stressed by deep water condition. Surveys were conducted before and after each drawdown events. It is not meant to be a controlled nor a continuing experiment. The District is currently preparing a new science plan which could include controlled studies.*

1544-1730: This appears to be one of the more comprehensive studies that the district is conducting and is in general an example of how to conduct more of the experiments. The section of the chapter is quite hard to follow due to an excessive use of acronyms and the use of cell numbers rather than a physical description of the plant type effect. The point appears to be differences induced by (primarily) SAV versus EAV dominant communities. Why not use that designation as opposed to cell location.

Response Line #1544-1730: *The section will be edited as suggested for clarity.*

1734: What about Fe and Ca in the sediments; these must have major roles in the release and trapping of P?

Response Line #1734: *The study presented in the chapter was a brief snapshot specific to vegetation and nutrient release response to dryout condition in STA-3/4. The authors agree that these elements play major roles in the release or sorption of P, primarily Calcium. These data are available and efforts to analyze various data gathered in the different STA cells, to relate them with phosphorus uptake performance, are continuing.*

1829-2066: Both of these studies appear to be of high quality and we look forward to seeing the more fully detailed papers that will ensue.

Response Line #1829-2066: *Thank you. The District will continue with this effort, including writing of fully detailed papers as studies are completed.*

Figure and table comments:

A generic comment on many figures is that the fonts for legends and axis labels are too small to read, often even when read on a monitor where they can be expanded in size quite easily. Printed

at 100%, there are virtually indecipherable. More egregious examples include Figs. 5.5, 5-23, 5-24, 5-25, 5-63

Response Fig. 5-6: *The authors will modify the figures to use larger fonts or expand the figure sizes to be more legible.*

Fig 5-6: The series are not labeled or defined.

Response Fig. 5-6: *The authors will apply labels to the series on the final chapter. Series 1 is for monthly TP concentration while Series 2 is for a 12-month moving average.*

Fig. 5.6 and other related figures – the manner of plotting the time series data doesn't make sense, at least the 12-month moving averages. These are normally plotted at the mid-point of the moving time period, i.e. at 6 or at 7 months for a 12-month moving mean. The first point is plotted at the 13th month suggesting that it is the average of the preceding 12 months, but the numbers don't seem to work. (See also the comment on Fig. 5-26)

Response Fig. 5.6 and other related figures: *The authors will change the terminology to “Preceding 12-Month FWM TP Concentration.” These values, which were calculated based on the preceding 12-month flows and TP concentration, help us track the 12-month period performance for evaluating permit compliance. Missing symbols in the plots mean that there were no flows during that period. When there is no flow, FWM TP concentration is not reported.*

Table 5-8: The font size changes on the last two lines

Response Table 5-8: *The authors will make changes in the final chapter.*

Fig 5-26, 5-34: There are a few months without symbols for FWM TP data. It could be the data was simply omitted or maybe they were months in which there was no outflow? If the latter, this should be noted in the caption. Additionally, how is zero flow accounted for in the calculation of a 12 month moving average? Several options exist and the one taken should be noted.

Response Figs. 5-26, 5-34: *The authors will change the terminology to “Preceding 12-Month FWM TP.” These values, which were calculated based on the preceding 12-month flows and TP concentration, help us track the 12-month period performance for evaluating permit compliance. Missing symbols in the plots mean that there were no flows during that period. When there is no flow, FWM TP concentration is not reported.*

Fig 5-31: This figure clearly demonstrates the improvement to SAV coverage when a grid the emergent vegetation is put across SAV cells. Is it feasible to continue this practice in all SAV cells? What timeline is considered for this type of improvement?

Response Fig. 5-31: *District efforts to add vegetation strips in SAV cells began in 2005, and since then, most of the SAV cells have vegetation strips in various configurations. The District has not set a specific timelines, but the Vegetation Management group has been continuing to implement this for further SAV cell compartmentalization. It is anticipated that the efforts will continue as needed based on cell condition and performance.*

Fig 5-36: The legend should read STA5 Cell 3.

Response Fig. 5-36: *The correct legend is STA-6 Cell 3. The authors will apply this change in the final chapter.*

Table 5-11: What is “section 2”?

Response Table 5-11: *Section 2 refers to STA-6 Cell 6-2. The authors will clarify this in the final chapter.*

Table 5-12: This is a nice way of presenting the various scientific studies underway. Also SRP was measured in some of the applied scientific studies but never in the routine monitoring?

Response Table 5-12: *The District has been measuring SRP on routine samples as well, but does not always report them annually in the SFER. This and other parameters have been reported in earlier versions of SFER. The focus of agency reporting has been TP, but the authors agree that in order to make further analysis and interpretation on STA performance, the agency will need to begin reporting relevant parameters. Authors will plan on this for next year's SFER.*

Table 5-13 - TP settling velocity of 12 m/yr is exactly the historic literature value for lakes. Curious coincidence or universal constant?

Response Table 5-13: *It is likely a coincidence. Variable settling rates have been observed for different STA areas and time periods.*

Figure 5-43: The figure caption is confusing. Though ultimately deciphered, a sw orded t he caption implies there are three transects when there are obviously more.

Response Figure 5-43: *The authors will modify the caption in the final chapter for clarity.*

Editorial comments by line number:

411; change *STAIE* to *STAIW*

Response Line #411: *The authors will apply the appropriate correction in the final chapter. The sentence should read: "The condition in STA-IE, which resulted in flow-ways being off-line or under restricted operation, as discussed previously, has also impacted the hydraulic and nutrient loadings in STA-IW."*

511: change *than any of the other STAs* to *of any STAs*

Response Line #551: *The authors did not see the phrase quoted in Line #511, but found this in Line #551. The authors will change to "STA-2 had the lowest outflow TP concentration (12 ppb FWM) among all the STAs..."*

607, 615: I believe 2001 should be changed to 2012

Response Line #607&615: *The wet season references pertained to calendar year 2011. Corrections will be applied in the final chapter to clarify.*

613: Obvious typo here.

Response Line #613: *Correction will be applied in the final chapter.*

780: change *and to to*

Response Line #780: *Correction will be applied in the final chapter.*

788: causing *the* previously desiccated

Response Line #788: *Correction will be applied in the final chapter.*

1101: Change *degraded* to *lowered*

Response Line #1101: *The main purpose of the activity was to obtain material from the high pad area to fill a slough within Cell 1A of STA-5. The sentence will be revised to: "This area was scraped in WY2010 to obtain fill material for a slough area in Cell 1A".*

1172: Figure 5-41

Response Line #1172: *Figure # will be corrected in the final chapter.*

1209: rating *curve* was

Response Line #1209: *The authors will use the term: "rating equation."*

1274: Should this be *areal*?

Response Line #1274: *Yes, edit will be applied in the final chapter.*

1476 and 1487: Some typos in here.

Response Line #1476 & 1487: *The authors will apply the appropriate edits in the final chapter.*

1578: Figure 5-53

Response Line #1578: *Figure caption will be corrected in the final chapter. The correct caption is: **Figure 5-53. Locations of spatial soil sampling and sampling for the P stability study in STA-2 Cells.***

1579: use ft. rather than ‘

Response Line #1579: *Correction will be applied in the final chapter.*

Summary and Recommendations:

For the most part, this chapter is a straight-forward accounting of what has happened in the past year with regard to the stormwater treatment areas. In terms of the results achieved, there seems to be very little that is controversial. At most of the sites, results have been adequate, while two sites performed poorly and did not achieve their TP target levels. It is important to understand why these two failed and how future operation might improve performance and/or maintain it in the long run. While the chapter fulfills the requirements for an accountability review, the presentation leaves something to be desired from the Technical review perspective.

Response to Summary and Recommendations Part 1: *The reviewers are correct in that the chapter focuses on accounting of what happened in the water year as they relate to the water year performance. The draft chapter discussed one STA that failed to meet its interim effluent limit, which is STA-6. The rest of the STAs met their interim effluent limits. For STA-6, Lines 142-145, 921-926, and lines 950-963 addressed the reason why STA-6 did not meet the interim limit. Specifically, lines 950-963 states:*

PERMIT-RELATED PERFORMANCE ISSUES AND ACTION PLANS

In WY2012, STA-6 is the only STA that did not meet the interim effluent limit specified in the EFA permit. Due to the regional drought and lack of an efficient way to bring supplemental water to STA-6, the operational cells in this STA, i.e., Cells 3 and 5, dried out during the drought period beginning in October 2010 to July 2011, and again from December 2011 to the end of WY2012. Consequently, extremely high TP values (greater than 300 ppb at initiation of flow) were observed upon resumption of flow in July 2012, as a result of P flux from the oxidized soil. The twelve-month moving average TP concentration shows that the trend was slowly decreasing toward the end of the water year; however, TP spikes are anticipated again upon rehydration. Interim measures includes a gradual hydration of the cell, with no flow-through (no discharge), until there are indications of stabilization of TP levels within the cell. Once Compartment C is operational, it is anticipated that flow can be distributed more evenly among the eight flow-ways that now comprise the STA-5/6 flow path. The added capacity may help prevent discharging from a flow-way immediately after rehydration.

The panel believes it is time to carve out a more manageable experimental system somewhere within the STA system where the scientific approach can be applied to replicated experimental units large enough to be representative of the field scale, but small enough to not be influenced by currently uncontrollable variables such as hydrology (water depth, dry out) soil type etc. Data from these experimental units would provide a baseline of “best achievable” TP removal results and used to optimize controllable inputs such as vegetation type, hydroperiod, water depth etc. Though the initial cost of such a system might be high compared to continued annual expenditures of monitoring of current full scale system, it seems highly unlikely the current approach of monitoring such a non-expansive, variable system, even if conducted over another decade or more, will ever produce the desired output of knowing what controllable factors can be manipulated to optimize performance. Monitoring of a controllable experimental unit, with

multiple cell sizes on the order of a few acres, would ultimately lead to better operational decision making.

Response to Summary and Conclusions Part 2: *The authors continue to make improvements to the chapter based on previous peer review comments. Comprehensive data analysis effort is continuing and the results will be included on next year's SFER. As noted earlier, since most of the data were from non-controlled experiments, further analyses of data has been challenging due to large data variability and inherent variabilities among the different STAs and among the different cells within each STAs. The authors also believe that there is missing information, such as those related to biogeochemical processes, which could help with the cause and effect information that the reviewers seem to look for. A large amount of resources have been spent in earlier years, including evaluation of different treatment technologies. Field-scale experiments will require large areas and tremendous amount of resources, which has been scarce in the past few years. Currently, the District is developing a science plan to focus on information gaps with the goal of finding ways to further improve STA performance. Some of those studies could be controlled studies that the peer reviewer is looking for.*

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 6

Everglades Systems Assessment Section

Level of Panel Review: Technical

Reviewers: W. Dodds (AA); P. Dillon (A)

Posted: 10/10/12 @ 10:14 AM EST

***Comment #1:** This is a fairly thorough and well written chapter. The organization is clear and the overall amount of information presented is good. The document puts the current year in perspective with past years, giving strength and context to any management decisions.*

Response #1: We appreciate this statement and recognize the value of looking at each year with the context of previous years, historical conditions and current management practices.

Dillon General Comments

***Comment #2:** This chapter contains a great deal of useful and interesting data and is I think better organized than the comparable chapters in earlier reports that I have read.*

Response #2: We appreciate this comment too and suggest that the appearance of better organization is probably due to the format consistency applied across each ecological topic.

***Comment #3:** The overview provided in Table 6-1 is particularly useful and should be a feature of each annual report. Another excellent change this year is consideration of the past 4 years with respect to hydrology rather than just the past year; in my opinion, this should have been the norm as it is clear that parameters such as water table elevation are affected by longer-term hydrology than a single year. This kind of multi-year trend analysis should be used to assess the role of hydrology with respect to many other aspects related to the status of the ecosystem, both in this chapter and in other chapters.*

Response #3: Table 6-1 has been a feature of Chapter 6 for about the last 5 years. We agree that this is a very important feature. The fact that more long-term hydrology data is presented in SFER2013 is a departure from our instructions to stay focused upon one water year. We are not sure that this level of synthesis will be possible in future years.

***Comment #4:** I have two general comments about the chapter that would improve the reporting in subsequent years. The first is that the importance of hydrology, specifically drought episodes and major storm events, is apparent. These events control the most significant chemical and biological changes in the Everglades and surrounding areas. Climate change is certain to change the frequency and magnitude of these changes. It would be valuable to undertake a study to come up with some idea of what those changes are likely to be because they are almost certain to have major impacts on the Everglades. There are methods available to downscale the GCMs to local areas; the data needed for the downscaling clearly exist for south Florida. Various standard scenarios exist based on changes in emission of greenhouse gases which can be coupled with the downscaling to give an idea of what the expectations are for the future for key hydrologic parameters. Some of the targets for various ecosystem properties may need to be reconsidered in this light; they may not be feasible under future climate scenarios. In short, there is a need to be proactive rather than reactive to this issue.*

Response #4: We completely agree and the District Modeling Department has been exploring different downscale methods for dealing with Florida's unique geomorphic position. We might see this reported in next year's SFER

Comment #5: *The second point is that the data presentation would be much stronger in many places if there were some error estimates made. For example, the nesting bird data are interesting and in some cases the year-to-year changes are so great that there can be no doubt that major alterations have occurred. In other cases, it is unclear whether the reported changes (e.g. line 448, 9% decline in great egret nesting) are within measurement error. I'm sure that methods exist for estimating the error associated with these measurements.*

Response #5: In general, we strive to include error estimates whenever possible. However, sometimes we are asked to report on parameters that were specified by CERP, RECOVER or the Everglades Forever Act in ways that Management and Legislators have seen and understood in the past.

The District agrees with the Reviewer's concern regarding error estimates from aerial surveys. The first thing to note is that nesting population estimates reported here are the sum of multiple independent counts conducted by various agencies and universities throughout South Florida which currently lack a standardized sampling methodology. The level of precision of the estimates therefore varies among surveys, is largely unknown and is out of District control.

Nonetheless, researchers from the University of Florida have invested considerable efforts towards understanding the sources of these errors and how to estimate them. There are several kinds of error, probably additive or even multiplicative, that include 1) observer counting error (the variation among observers in counting nests that are plainly visible), 2) visibility bias (not seeing all the nests that are present because many are occluded by vegetation or angle of viewing) and 3) mis-estimation of the population of nest starts (this derives primarily from not detecting nests that start and stop between surveys, and confusing failed or finished nests with new nests). The first source of error is currently being measured using double observer approaches and photographs but there are not yet sufficient years of data to estimate this error. The second source of error can be estimated by comparing aerial survey counts with those of highly labor intensive ground surveys. Such comparisons have shown that visibility bias varies considerably among colonies in space and within colonies over time and one cannot simply derive correction factors that apply to more than one nesting season or one colony. Thus estimations of this source of error over the spatial and temporal scales associated with Everglades restoration are largely impractical. The third source of error is an enormous contributor to overall error in estimating the true number of nest starts in any season. Indeed, we may be underestimating the numbers of nest starts by 47 – 380%. This is independent of the two other sources of error, and again appears to be highly dependent upon year and colony. However, we are generally more interested in the numbers of breeding birds rather than nest starts so this error is generally considered less important than the previous two.

Ultimately, we are not yet at the stage where we can measure error with sufficient precision over space and time. Solving this issue can only be achieved by following individual nests in a sufficient number of colonies throughout the entire season. Current studies are investigating the use of robotic aircraft that can take high resolution photos that are also accurately georeferenced. This essentially allows for geoidentification of large number of individual nests over sequential flights, following them through time to produce turnover estimates and eventually an estimate of the seasonal numbers of nest starts. Such work will hopefully provide sufficient precision estimates for these populations in the near future.

Specific Comments Dodds and Dillon Combined

Comment #6:

In general the summaries avoid giving specific numbers, and might be improved with at least a few more example numbers.

Response #6: We agree that adding specific numbers to the summaries is a good idea.

Comment #7: line 6 - what is the difference between landscape and ecosystem ecology?

Response #7: The difference between ecosystem and landscape is basically the scale of the discussions. Line 6 will be edited to read: “The studies and findings discussed in this chapter are presented within four main fields: (1) wildlife ecology, (2) plant ecology, (3) ecosystem ecology, and (4) landscape patterns and ecology.”

Comment #8: Line 20 page 6-1 “These conditions were positive for the Everglades and may indicate that local meteorological influence on rainfall patterns may be beneficial despite global circulation patterns 20 that would point toward drier conditions for South Florida.” This is a strong statement that might need to be qualified or removed. Does this mean that biological and chemical influences of overall drier conditions that are predicted by regional application of global climate models may be mitigated by more intense storms?

Response #8: This is too strong a statement to use at this time and will be removed. However, it was meant to convey the importance of sea breezes and land-sea interactions which, at the moment, appears to buffer against high temperatures and low rainfall predicted by many global climate models.

Comment #9: Line 146 6-6 These statements are a bit on the non-technical side. What is a fantastic year? An average % population increase could put this in perspective.

Response #9: Change this: In review, the WY2009 drought was a fantastic year for many species of wading birds, WY2010 flooding was a terrible year for most wading birds, the WY2011 drought was not a good year, and WY2012 was good for some species, but bad for most.

To This: In review, WY2009 was initially wet followed by a constant, rapid water-level recession which led to one of the best nesting seasons on record in the Everglades; WY2010 was relatively wet throughout and a relatively poor nesting year; WY2011 was initially dry with rapid recession rates and drying conditions that were initially excellent but too short-lived for successful nesting; and WY2012 was initially wet with optimal recession rates but numerous rain-driven water level reversals and a poor prey base (due to a dry WY2011) contributed to a third successive year of poor nesting.

Comment #10: Figures 6-1 to 6-7 are nice figures and really lay things out well. They also highlight the benefits to longer-term analyses of data.

Response #10: Thank you

Comment #11: line 357 – What is the rationale for these exceedances of the Florida ay MFL criteria. These are largely natural events so it is not clear why it should be considered an exceedance. Is it an exceedance because measures are supposed to be taken relating to managing flows that would prevent this?

Response #11: These exceedance thresholds are established based on the analysis of a 31-year record of historical data and determined to be outside the range of natural variability, i.e. the result of human activity. In the event of a violation of the threshold, flows are to be managed so as to alleviate the condition. This explanation will be inserted into the document.

Comment #12: The pilot experiments using enclosures for fish movement are promising. Using enclosures of this small size could cause problems if that is what is planned for the actual studies. There should be fairly good data on ranges of movement for these species, if not, some larger scale measurements would be warranted. Given that the few fish tested moved freely throughout the enclosures, it is possible that the enclosures constrained the animals or offered cover where it would not normally occur.

Response #12: As with any manipulative approach, it remains a challenge to make inferences about ecological dynamics operating at scales broader than the observational. Biases in small-scale experiments may limit our ability to extrapolate results to natural ecosystems of interest. But, if the response variables being measured are dominated by within-patch processes (e.g., local

water level and habitat availability, and/or individual movement decisions), results from small scale experiments should scale up to match larger-scale processes. Thus, we expect that the behaviors quantified in this type of large field enclosure setting containing natural habitat features and tracked by antennas that minimize observer effects provides unusually-detailed and insightful behavioral data that are quite rare in the aquatic realm. We did not observe any evidence (that fish movement was unnaturally restricted (e.g., extremely high movement rates). Further, movement rates are notoriously leptokurtic, with most individuals making small movements while only a few individuals make larger movements. Thus, we believe that the scale of our enclosure is relevant to the scale at which most small fishes move (e.g., 10s of meters). Lastly, in our system and in others, seasonal changes in water levels along with variation in microtopography likely limit the range of movements fishes make, forcing fish to make small movements to local water maxima within or near range when water levels recede. Although, restrictions imposed here by the enclosure are artificial, they are to some extent reflective of natural conditions.

Comment #13: *The germination studies are good and well described*

Response #13: Thank you.

Comment #14: *line 727 – again, another place where error estimates are needed to determine if this is a real change or a measurement artifact. The fact that 20 species disappeared in a year suggests that this is dependent on the intensity of sampling.*

Response #14: An error estimate cannot be derived for species additions and subtractions in this context. Error estimates are provided for the alpha-diversity, which can be quantified for individual plots, but cannot be quantified for a cross-plot total.

Comment #15: *line 958. If cattails provide refuge from predators this would also explain these results.*

Response #15: We agree that the ability of cattail to provide a refugia from predators could also explain the results, and was part of the rationale for our statement that greater prey availability may occur at the plot edges.

Comment #16: *line 1195. Beta diversity is simply a comparison among habitats. The index may reflect temporal patterns using space for time substitution. This needs to be a bit more clearly explained here. Also if species identities matter, then clustering methods or PCA might be a better choice for analyses.*

Response #16: Along the transect species turnover was represented by the B-C dissimilarity between two adjacent segments of sites in split moving-window (SMW) boundary analysis. To examine the relationship between the degree of species turnover and the environment gradient, habitat heterogeneity was calculated as the mean absolute difference in values for elevation (and its covariates hydroperiod and water depth), and soil depth. To maintain consistency between normalized B-C dissimilarities (Z-score) and habitat heterogeneity, we first calculated absolute mean difference in the values of environmental gradient variables averaged over the sites present in each of four window sizes and then averaged the values for each mid-point for the four window sizes. Beta diversity ($\beta = \gamma/\alpha$) was also calculated to represent overall species turnover along the gradient on each transect. To quantify overall habitat heterogeneity along transects, we calculated coefficient of variation (CV) for elevation, hydroperiod, water depth, and soil depth. Finally, multiple-regression was used to quantify the relationships between species turnover and variability in elevation, hydrologic parameters, and soil depth within and across transects.

The reviewer suggests using clustering methods or PCA for the analysis. We used a Non-metric multidimensional scaling (NMDS) ordination to examine the relationship between species composition and environmental variables. However, due to limitation of space in the whole report, we left this analysis out of the current S FER. Adding this analysis, it would include another figure and one page to describe the results, analysis, and discussion.

Comment #17: line 1271 – *It might be better to collect integrated samples over whatever the depth at the sampling site is rather than at 0.5 m only, which may not be representative of the water column at least at deeper sites.*

Response #17: The collection at 0.5 m adheres to the District Field Sampling Quality Manual procedures for collection of surface water samples following FDEP guidelines. The average depth of Florida Bay is less than 1 meter. While there may be some deeper sites where this method would be preferred, our ability to perform comparisons across the network may be compromised.

Comment #18: line 1282 *This is an awkward sentence.*

Response #18: This sentence will be changed to: Chlorophyll a concentrations exhibited high monthly variability within some regions and contrasting trends between regions.

Comment #19: line 1332 and fig 6-31 – *would be much more useful in absolute units rather than as relative fluorescence values; I thought ground-based measurements were available that would allow calibration of the RFU?*

Response #19: Agreed. At the time the document was written, the calibration data had not been fully analyzed but it has since become available and the figure will be updated.

Comment #20: line 1440 – *Generally, sediment core incubations rarely provide unequivocal data; there is always some disturbance of the surface sediments, the overlying water, etc. The very short (4 hr) incubations may reduce these problems, or may make them worse.*

Response #20: Agreed. However, the objective of this study is to make an initial assessment of the magnitude of sedimentary nutrient sources. No other nutrient flux data exists in these regions and this is the most efficient method for this assessment.

Comment #21: *Figure 6-35 A panel with N to P ratios would be nice here. This might be important as it could indicate temporal changes in N and P limitations.*

Response #21: Excellent point- such a figure will be developed, contingent on space being available for it.

Comment #22: *Figure 6-37. An analysis of correlation between ammonium flux and oxygen flux would be interesting.*

Response #22: Agreed. This information will be added.

Comment #23: 6-39. *The “3d” format in excel plots is not very professional.*

Response #23: Plot will be replaced.

Comment #24: *Figure 6-45. Nicely illustrates major points.*

Response #24: Thank you.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 7

LeRoy Rodgers

Level of Panel Review: Accountability

Reviewer: W. Dodds (AA)

Posted: 10/10/12 @ 10:19 AM EST

***Comment #1:** This chapter is well done. I do not have many comments. It seems as if SFWMD is doing what they can to control non-native species. It is especially good to see cross agency coordination on control of some species. As the chapter is mostly descriptive, there is very little to comment on here other than it is very clear and gives the information in an easy to access format. The chapter serves as an excellent reference for some of the other chapters.*

Response #1: The authors appreciate the reviewer's comments. As noted in the text, invasive species remain an important issue for region-wide restoration efforts. The purpose of the chapter is to provide an update on the status of invasive species that have the highest likelihood of negatively impacting Everglades restoration objectives. We hope that this chapter will continue to provide scientists and land managers throughout the restoration footprint with pertinent information on invasive species. While the authors are encouraged by recent successes of some programs (e.g., District's Melaleuca Control Program, the interagency biological control program, and several successful rapid response programs), concern remains over the immense regional challenges of several highly invasive plant and animal species.

As the reviewer noted, the District and other agencies place a great deal of emphasis on coordination and collaboration towards addressing the threat of invasive species. This is particularly important in the current budget environment. Land managers and invasive species scientists continue to face a growing list of invasive species threats as funding remains stagnant or decreases. Interagency coordination efforts (e.g. Everglades Cooperative Invasive Species Management Area) are proven means of increasing the efficiency of invasive species management activities and help to promote regional strategies with the highest likelihood of success.

Given the documented impacts of invasive non-indigenous species, the District and partner agencies engaged in Everglades restoration must remain committed to further research and management efforts. Since few invasive species are ever eradicated once established, a long-term commitment to "maintenance control" of successfully managed species is necessary to avoid re-establishment and costly retreatment efforts. Perhaps more importantly, the agencies must sharpen their focus on rapid response of new introductions. After preventative regulations, early efforts to control newly detected non-indigenous species is the most cost-effective means of dealing with biological invasions.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 8

Joyce Zhang and Bruce Sharfstein

Level of Panel Review: Technical

Reviewers: P. Dillon (AA); W. Dodds (A)

Posted: 10/9/12 @ 7:36 AM EST

***Comment #1:** In general, the chapter is well-written, with clear and relatively concise writing. The results are explained in adequate detail. The conclusions drawn are supported by the data presented. In summary, I thought that this was the best version of the chapter on Lake Okeechobee that I have read since I have been a reviewer of the report.*

Response #1: Comment appreciated.

***Comment #2:** The greatest concern related to this section as remains the progress made towards reducing the TP to the target value of 140 metric tons/year. Despite very substantial efforts to reduce loads, many of which have had significant success, and many of which have cost large amounts, the total phosphorus load is in the same ballpark as it has been for the past several years. I understand that the target load is a legislated requirement, but in the end, it may simply not be feasible, and if not, it may be time to, in effect, start re-negotiations. Unless a clear path to meeting this target load is apparent (and described in detail in the next year's report), then realistic scenarios for the future should be developed. This is particularly relevant since the potential for very large internal P loads makes the desired changes in water quality even more difficult. I would encourage more intensive efforts in future to quantify the nutrient load-trophic status response relationships through modeling efforts so that what to expect with different target loads could be identified clearly.*

Response #2: There have been substantial efforts to reduce P loading to the lake with little change in the actual loading to the lake. This is thought to be a function of a delay in the response time of these efforts due to legacy phosphorus in the watershed. As pointed out in comment #3 below, in lake TP concentrations have shown some improvements in recent years and are actually the lowest since 1993. The reviewers' suggestion to quantify the nutrient load-trophic status response relationships through modeling efforts is appreciated and this information will be forwarded to the coordinating agencies (SFWMD, FDEP, and FDACS) for further consideration. Both reduction programs and understandings of nutrient load-trophic status response relationships will be refined as the program is implemented.

***Comment #3:** On the positive side, the lake TP concentration has shown some recent improvement. Although the values reported for 2011-2012 are still well into the hyper-eutrophic category, they are lower than previous years. The five-year moving average, however, (Figure 8-14) is somewhat less promising with the mean still higher than 15 years ago. The complex mix of hydrology and nutrient dynamics explains the high load-lower concentration situation, and I'm sure that this is/was predictable with the appropriate models. It is also a good reason to continue with using 5-year average values for many parameters.*

Response #3: Comment appreciated.

***Comment #4:** Hydrologic events, particularly storm events and drought episodes, are clearly of great importance in determining both the external loads (and probably the internal loads) and the lake's response to these loads in terms of trophic status parameters such as chlorophyll concentration, water clarity, etc. Climate change is certain to change the frequency and*

magnitude of these events. It would be valuable to undertake a study to come up with some idea of what those changes are likely to be because they are almost certain to have major impacts on the lake. There are methods available to downscale the GCMs to local areas; the data needed for the downscaling clearly exist for south Florida. Various standard scenarios exist based on changes in emission of greenhouse gases which can be coupled with the downscaling to give an idea of what the expectations are for the future for key hydrologic parameters. Some of the targets for the lake's trophic status properties may need to be reconsidered in this light; they may not be feasible under future climate scenarios. In short, there is a need to be proactive rather than reactive to this issue.

Response #4: The authors strongly agree. The District is developing both a periphyton and an emergent aquatic vegetation sub-module of the Lake Okeechobee Environmental Model (LOEM) to complement the recently completed and validated submerged aquatic vegetation sub-module. These three sub-modules along with the Lake Okeechobee Water Quality Model (LOWQM) will make it possible to model ecological responses to different hydrological regimes whether they are related to climate change or variations in multi-decadal climate oscillations.

Comment #5: In summary, it is obvious that a great deal of high quality data have been collected on the lake and its tributaries, and that these data have been described and interpreted clearly and correctly in almost all cases. It would however be beneficial to include more in-depth analysis particularly with more conclusions drawn in the next report. A number of remedial projects have also been clearly presented; most of these have been successful to some extent. A brief indication of where these watershed projects are going in future and what their ultimate potential is in terms of TP reductions would be useful, although I accept that funding considerations largely dictate this.

Response #5: In-depth analysis of water quality data is expected to be included in next year's SFER. Trend analysis results and the overall watershed project evaluation are expected to be documented in the Lake Okeechobee Watershed Protection Plan update which will be completed in early 2014.

Comment #6: line 111. Blue green algae is an old fashioned term, cyanobacteria should be used.

Response #6: Agreed, the final chapter will be revised as suggested.

Comment #7: line 129. Scientific names of all species should be used at least first mention in the document.

Response #7: Agreed, the scientific names for the listed species will be inserted into the text beginning at line 129.

Comment #8: line 195. Exotic plants means non-native?

Response #8: Yes. The authors tend to distinguish between native plants, exotic plants (non-native), invasive exotics (those exotics that have the potential to spread rapidly and overtake native plant communities) and nuisance plants (natives which under the correct conditions can replace the natural vegetation mosaic with dense monocultures).

Comment #9: line 338. The document jumps between metric and English units here and elsewhere, eg. acres, and should stick to metric units

Response #9: Agreed, this will be revised in the final chapter. The metric and English unit conversion table also will be included in the final report, consistent with previous SFER reporting.

Comment #10: line 366. The potential for nutrient removal by macrophyte harvesting has been raised in the past. Macrophyte harvesting must be less expensive than dredging. Furthermore, particular areas with undesirable plant species could be targeted. My understanding is that repeated harvests not only remove significant nutrients but also leads to deterioration in the targeted plant's condition.

Response #10: While some small percentage of the lake's floating macrophytes are removed by mechanical harvesting each year, the effort is primarily directed at maintaining navigational access in narrow channels. However, mounting a larger program for the purposes of nutrient control is problematic for several reasons: (1) the lake is very large and the need to transport harvested plants from remote work sites to the nearest road access point is time consuming and economically unattractive; (2) phosphorus concentrations of living plant tissue tend to be less than 1%, so large quantities of biomass need to be moved to achieve relatively modest TP removal although substantial amounts of organic carbon can be removed in this manner; and (3) once removed from the lake, the harvested macrophytes need to be either moved out of the watershed or sequestered in a manner that prevents their nutrient content from returning to the lake via runoff or groundwater leaching, a potentially costly undertaking.

Comment #11: *fig 8.7. The approach is very interesting here, and seems valuable. The actual years need to be defined in the legend.*

Response #11: Agreed, the period definition will be added in the final chapter as follows: Period 1: November 21, 2008–November 20, 2009; Period 2: March 9, 2010–March 8, 2011; Period 3: March 9, 2011–September 30, 2011; Period 4: November 13, 2011–March 14, 2012 for grassy site and October 1, 2011–June 20, 2012 for all other sites.

Comment #12: *line 565. The delay in implementation makes no apparent sense. Could the rationale for this be explained more fully in terms of what the delay could achieve.*

Response #12: Yes, the following text will be included in the final chapter for clarification:

Fisheating Creek is the only tributary with an uncontrolled discharge point to the lake (i.e., there are no structures on Fisheating Creek directly controlling discharge to the lake). It is characterized by extremely flashy flows and is one of the major sources of TP loading to Lake Okeechobee (SFWMD et al., 2011)³. The Fisheating Creek Feasibility Study involves formulation, evaluation, and selection of the most appropriate mix of storage and water quality features to improve hydrology and water quality in the Fisheating Creek Sub-watershed. Planning targets for achieving surface water storage and quality improvements (TP load reduction) were also established through analyzing pre-drainage and existing conditions outputs from WAM simulations in close coordination with stakeholders and other agencies. The next step is to locate conceptual water quality and storage features. The Natural Resources Conservation Service is currently developing the Fisheating Creek Special Wetland Reserve Project (WRP), which involves large tracts of lands located north of State Road 70 that account for approximately 18 percent of the total sub-watershed area. It is important to account for all upcoming hydrological improvement projects in the Fisheating Creek Watershed in order to adequately characterize the additional features that will be needed to meet study goals. Postponing the study until WRP details are available and incorporating them into the FEC FS will allow this to occur. The USDA-NRCS plans to have the necessary data available in 2013, and the District expects to resume the project in FY2014 once this information is available.

Comment #13: *Tables 8.1 and 8.2. There are too many significant figures in these tables. A maximum of 3 is justified here. Also, a figure demonstrating these changes would be a good addition.*

Response #13: Table 8-1 contains three significant figures; Table 8-2 has four significant figures in order to be consistent with the Budget on Table 8-13. Figures 8-13 and 16 display the changes of the TP and TN load from 1973 to present.

³ SFWMD, FDEP and FDACS. 2011. Lake Okeechobee Protection Plan Update. South Florida Water Management District, West Palm Beach, FL; Florida Department of Environmental Protection, Tallahassee, FL; and Florida Department of Agriculture and Consumer Services, Tallahassee, FL. Final Report Final Report available online at http://www.sfwmd.gov/portal/page/portal/xrepository/sfwmd_repository_pdf/lopp_update_2011.pdf.

Comment #14: line 622. *An in-lake goal for TN would be useful. While P is probably limiting in most parts of this lake, there are clear effects of alteration of TN:TP ratios, including dominance of certain types of cyanobacteria, some of them potentially toxic. If maintaining biotic integrity is a goal, getting close to historic conditions of TN as well as TP is more likely to help reach that goal.*

Response #14: The causative pollutant for the Lake Okeechobee impairment is TP; therefore a Lake Okeechobee TMDL only exists for TP. There is, however, a TN:TP ratio goal of >22:1 for the lake (see Table 8-11) which address your concern regarding the alteration of TN:TP ratios. Note that there has not been a trend in nitrogen since 1983. The average offshore value was 1.6 mg/l±0.3 since 1983, nearshore values have averaged 1.5 mg/l±0.15 since 1987 (from James, R. T., Gardner, W., McCarthy, M. & Carini, S., 2011: Nitrogen dynamics in Lake Okeechobee: forms, functions, and changes. – *Hydrobiologia*. 669:199-212.) this average value is very similar to the 1973-1980 average (see fig 3a. James, R. T., Smith, V. H. & Jones, B. L., 1995: Historical trends in the Lake Okeechobee ecosystem III. Water quality. – *Archiv für Hydrobiologie. Suppl.* 107:49-69). The authors agree with the reviewer that getting closer to historic conditions of TN and TP will help maintain biotic integrity for the lake.

Comment #15: line 721. *The huge flux during the storm leads to the need to evaluate climate change and the predictions of more frequent extreme events.*

Response #15: The scientists at the District believe science on climate extremes at regional scales in Florida is not sufficient to make reliable projections. But, much research is underway (Irizarry-Ortiz, M.M. et al., 2011: Historical trends in Florida temperature and precipitation, Hydrol. Process. Published online in Wiley Online Library).

Comment #16: table 8-5. *The TP concentrations at some sites are higher than treated sewage. Although it sounds extreme, it may be viable to treat some of these inflows as such, with full-scale tertiary treatment. It is at least worth considering the cost relative to the other methodologies that are being attempted.*

Response #16: These canals drain large areas with various land uses. High TP concentrations in canal water which capture stormwater runoff are unfortunately not uncommon in the Lake Okeechobee drainage basins due to the legacy phosphorus issue. Implementing tertiary treatment at inflow points would be very challenging for several reasons (including, but not limited to, the variability of flow volumes), would likely be very expensive and would not help control the release of pollutants at the source. The authors concur that some additional BMPs or perhaps dredging of soils would be helpful if conditions permit and funds are available in the future.

Comment #17: line 776. *A non parametric pair wise comparison technique throws out most of the temporal information. There is also concern that running multiple pair wise comparisons is not corrected for repeated measures, e.g. Bonferroni.*

Response #17: In the final chapter, the statement will be clarified as “A Mann-Whitney test was used to compare concentrations levels between the two study periods at a significance level (α) of 0.05 (Tables 8 -8 through 8 -10).” This non -parametric test does not perform a pair wise comparison but rather compares data between two groups. No multiple pair wise comparisons were performed with the data. All trend analyses were determined using the seasonal Kendall test (a non-parametric test).

Comment #18: Table 8-11. *The TP load is still far from the target; this table shows clearly that loads are hydrologically driven which points to the use of mean volume weighted concentrations when looking at inflow contributions and changes over time*

Response #18: The reviewers are correct and the authors often take this approach when reviewing flows from individual structures or basins. However, as the lake TMDL is a load-based value that takes into consideration both flow and concentration, the agency typically expresses total lake inflows and outflows in terms of load.

Comment #19: line 867. *The calculation of sedimentation coefficient is excellent; this is useful information for modeling the response of the lake to its load.*

Response #19: Comment appreciated.

Comment #20: line 972. *Can this mapping be done more efficiently with remote sensing methods with some ground level controls rather than detailed ground level mapping?*

Response #20: Because many regions of Lake Okeechobee tend to be highly turbid or very dark and tannic, aerial remote sensing for SAV does not work well. There are several sonar-based applications that have worked successfully elsewhere in the system and might work on Lake Okeechobee. The possibility of conducting a pilot test for this work would be of interest if future priorities and funding permit.

Comment #21: line 998. *It is good to see the attempts to standardize the SAV measurements; such efforts are most useful and if there are other data sets for other parameters where this can be done, I would strongly encourage it. Ultimately, the long-term nature of the data sets for this ecosystem will be invaluable.*

Response #21: Comment appreciated. The agency recognizes the value of these long-term data sets and actively works to ensure methodological consistency from year to year, including conducting periodic reviews of monitoring methodology and data tabulation and analysis.

Comment #22: line 1194. *Has there been any analysis of wind with respect to bloom formation? Calm conditions should favor cyanobacterial blooms.*

Response #22: Generally, blooms are more frequent during the summer months and winds across the lake are almost always lighter in the summer than in the winter. There is also some anecdotal evidence that indicates that blooms move around the lake in response to prevailing winds. However, as bloom data is collected at monthly intervals at only six stations lake-wide, the resultant data set is probably not sufficiently robust to do a detailed analysis of wind speed and cyanobacterial bloom occurrence.

Comment #23: line 1204. *The section on phytoplankton is well done; on periphyton, a broader perspective is warranted. Questions such as how much does the periphyton contribute to the total algal biomass in the lake; is it important relative to the SAV in terms of nutrient uptake could and should be addressed.*

Response #23: This year's periphyton section was primarily intended to report on additional work completed during the past water year. The authors are currently developing a manuscript using a long-term periphyton data set that will attempt to answer the questions mentioned here among others. It is anticipated that this more detailed information will be included in next year's SFER. District scientists and collaborators are also working to develop a periphyton sub-module of the LOEM (Lake Okeechobee Environmental Model) to help predict periphyton nutrient storage and related parameters under varying Lake Okeechobee hydrologic scenarios.

Comment #24: line 1331. *If I understand the methods used for the fish trawl, this was done only once in the year. This is probably not very good data to compare year-to-year, at least not without some estimate of within-year variability.*

Response #24: The reviewers are correct that it would probably be better to do this monitoring with increased frequency. However, the sampling is done at the same time each year to try and maintain inter-year consistency. It should also be noted that this work is done and funded solely by the Florida Fish and Wildlife Conservation Commission so the District has limitations in its ability to supply methodological input for such efforts.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 9

Bradley L. Jones, David H. Anderson,
Stephen G. Bousquin, Michael D. Cheek,
David J. Colangelo and J. Lawrence Glenn III

Level of Panel Review: Technical

Reviewers: P. Dillon (AA); O. Stein (A)

Posted: 10/11/12 @ 9:56 AM EST

***Comment #1:** Table 9-3 provides a very valuable overview of what is being monitored and where to find information in previous reports. However an additional table using a similar format that spells out what each expectation number is would be equally valuable as these expectations very succinctly define the goals of the restoration project.*

Response #1: All KRREP metrics that are evaluating expectations in the SFER now include an expectation statement at the start of the status report; to avoid confusion, we are adding information at the start of the status reports for metrics that do not have expectations. Full descriptions of all of the restoration expectations, including their predicted values, are available at http://my.sfwmd.gov/portal/page/portal/xrepository/sfwmd_repository_pdf/krr_volii_expectations.pdf.

The table suggested by the reviewer would add at least a page to the chapter at a time when we are making efforts to reduce the length of chapters. Therefore, no change has been made in the chapter text.

***Comment #2:** One general problem is that there appears to be a potential conflict between some of the expectations of the Kissimmee River restoration project, specifically between expectations for the period of floodplain inundation (Expectation #3) and dissolved oxygen (Expectation #8). The data from this year's high runoff events clearly demonstrates that high flows after low flow periods decreases dissolved oxygen concentrations to below that for fish survival, a pattern seen previously (lines 339-352). Yet that diversity in the period of inundation is exactly what the project is trying to achieve. Are these expectations incompatible? If so does the District have the authority to alter some?*

Response #2: It is premature to conclude that the expectations for floodplain inundation (Expectation #3) and dissolved oxygen (DO) (Expectation #8) are incompatible for these reasons:

1) The occurrence of a low DO event like the one described in the chapter does not necessarily preclude the achievement of the DO expectation. The averaging for the DO expectations includes very short-lived events that may not be affecting the overall average much; however these short-lived events can have large impacts on fish. The expectations are not necessarily incompatible although these short lived sags in DO concentration are not desirable.

2) Expectation #3 for floodplain inundation is based on pre-channelization data, when fish, especially sunfish, were an important part of the system. Inundation of the floodplain for an extended period in most years prior to channelization is thought to have provided important off-channel habitat for many species of fish. While pre-channelization dissolved oxygen data are not available, concentrations must have been adequate to support sunfish.

3) It is important to recognize that the restoration project is still in complete and in an interim period. At the end of the restoration project, the headwaters revitalization schedule will be implemented, which will result in a flow regime that more closely approximates pre-

channelization conditions. If the low DO events are related to the flow regime, the new flow regime might reduce the severity and the frequency of such events.

4) Many factors can contribute to the occurrence of low DO events; we don't know what role, if any, that floodplain inundation plays. We recognize the importance of understanding the causes of low DO events for possible adaptive management to reduce their severity and frequency. As a first step, analyses of existing data are being conducted to examine the relationship between low DO events and such factors as rainfall, runoff, changes in discharge and water levels, and loads of nutrients and organic carbon. See also response to Comment #12.

No changes to the Chapter 9 text are needed.

Comment #3: *line 16-17: What are the long-term implications of the funding shortage and the reduction or loss of many of the non-KRRP projects?*

Response #3: The purpose of these non-KRRP activities was to fill gaps in existing ecological data sets that were identified as useful in refining evaluations of ecological response to hydrologic management/conditions in the Kissimmee Chain of Lakes. In some cases, new studies were proposed where little or no data exist. Other studies were proposed to augment spatial or temporal components of existing sampling protocols to increase the robustness of the data sets. Ecological response evaluations still will be feasible with data from continuing studies, but interpretation will remain at a coarser scale. In the chapter text, we will revise the statement to read, "Due to fiscal constraints, several non-KRRP-related studies in the Upper Kissimmee Basin have been postponed, reduced in scope, or discontinued; these changes are not anticipated to have long-term impacts on our ability to conduct lake evaluations."

Comment #4: *line 73: Climate change is almost certain to have major impacts on precipitation and temperature in south Florida. What are the consequences in terms of meeting expectations for the seasonal flow pattern and staying within the desired water levels? Some general comments on this would be valuable, although a detailed analysis probably requires a separate additional project.*

Response #4: We agree that climate change has the potential for major impacts on precipitation and temperature in South Florida. However, Comment #4 refers to text on line 73 that describes difficulties during the interim period in meeting Expectation #1 for continuous flow and Expectation #2 for variable flow. We believe these difficulties largely reflect the incomplete nature of the restoration project, specifically the limited potential for upstream water storage under the current interim schedule, rather than the effects of climate change. The interim period appears to have sufficient water to meet the expectations, which are based on pre-channelization data. This is indicated by nearly identical values and lack of statistical difference for mean annual discharge at S-65 of $34 \text{ m}^3/\text{s} + 7 \text{ SE}$ for the interim period and $35 \text{ m}^3/\text{s} + 4 \text{ SE}$ for the pre-channelization period. Because of the limited storage under the current schedule, much of the water discharged from the upper basin is made during flood control releases. Implementation of the headwaters revitalization schedule at the end of the restoration project will allow water levels to rise 1.5 ft above the maximum elevation of the current schedule, creating an additional 100,000 ac-ft of storage. This additional storage will allow water to be held longer in the upper basin and released under a more natural flow regime to meet the hydrologic requirements of the restoration project.

The first paragraph in the Hydrology section (line 441-449) will be revised to indicate that the interim schedule was not expected to provide the seasonal pattern of discharge needed for restoration.

Comment #5: *line 86: The oxygen results are very promising. This is a critical parameter in terms of many the biological communities in the basin, and it is important that the oxygen-related criteria were met or almost met despite the hydrologic conditions not being optimal at all times.*

Response #5: We agree with the reviewers' comment.

Comment #6: line 107: *The floodplain storage of phosphorus is very important. The measures being implemented to reduce the Lake Okeechobee phosphorus to its 140 metric ton target are not fully working and are not going to be successful. Unless some new approach or some different process that hasn't been considered takes effect, the loading target will not be met. This floodplain storage may be the only way the target can be met and so must be fully explored.*

Response #6: We agree that floodplain restoration could contribute significantly toward the reduction needed to meet the Lake Okeechobee phosphorus loading target. We are working toward understanding the floodplain assimilation processes better and estimating the floodplain's capacity to retain phosphorus. Because phosphorus loading is highly dependent on discharge through the river, the restored river-floodplain system may not retain enough phosphorus to help meet the Lake Okeechobee loading target during wet years.

No change to the chapter text is needed.

Comment #7: line 112: *The different response of nitrogen compared with phosphorus to the hydrologic events indicates different sources for the two nutrients. Some discussion of this is warranted.*

Response #7: The main point of lines 112-118 is that concentrations of total nitrogen (TN) in the C-38 canal do not vary as much as total phosphorus (TP). The text explains that TP appears to have sources in the watersheds of Pool A and the south end of Lake Kissimmee. Sources of TN have not been identified, but the lower variability of TN concentrations indicates that these sources are more diffuse. Also, the range of TN concentrations is typical of ambient concentrations in many other water bodies in south and central Florida, and may be indicative of the general physiographic characteristics of this region. These findings might be taken into account in determining management options for nitrogen runoff in the Kissimmee Basin.

No changes will be made to the chapter text, but we will consider further analysis and interpretation of these nitrogen results in the next SFER.

Comment #8: line 154: *Are the newer reduced ranges in water fluctuation due to lower peak stages, higher low stages or both? Also see comment on lines 204-207.*

Response #8: The text on Line 154 will be revised to indicate that both high and low stages were eliminated.

Comment #9: line 204-207: *Assuming flood control management limited maximum stages, increased storage would increase the extent of littoral zones. It is a little harder to envision how increasing minimum stage increases the littoral zone extent. Increasing storage by raising either maximum or minimum stage is dependent on increasing storage in the rainy versus dry season. Since two goals are to have discharge for 365 days per year in the restored lower section (Expectation #1) and to keep that section inundated for at least 180 days per year (Expectation #3) it would appear that more storage will be required in the dry season corresponding to higher minimum stages in the upper lakes. A little more explanation of how the new Headwaters Revitalization Schedule will increase the extent of upper lake littoral zones is warranted.*

Response #9: The Headwaters Revitalization Schedule was designed to meet the hydrologic needs of the Kissimmee River Restoration Project including continuous flow and floodplain hydroperiods. It creates an additional 100,000 ac-ft of storage by allowing the water levels in Lakes Tiger, Cypress, Hatchineha and Kissimmee to go 1.5 feet higher than is allowed by the current interim schedule. Under the current interim schedule, much of the water released at S-65 is made for flood control with rapid increases to larger discharges than needed for the restoration project followed by rapid decreases. The additional storage created by the headwaters schedule combined with a discharge schedule that varies with lake stage (higher discharges at higher stages) is expected to result in a discharge regime that more closely approximates the re-channelization regime and should result in the achievement of the hydrologic expectations. Raising the upper limit of the regulation schedule and holding water in the lakes for longer

periods of time has an incidental benefit of expanding the area of littoral wetlands, which was not a goal of the restoration project (USACE 1996). It has been estimated that implementation of the headwaters schedule will create 7,236 acres of seasonally inundated, short hydroperiod wetlands (USACE 1996).

Text on lines 204-207 will be revised as follows:

“Increasing storage in the headwater lakes by allowing higher stages for longer periods of time is expected to have the additional benefit of improving the quantity and quality of lake littoral zone habitat in Lakes Kissimmee, Hatchineha, Tiger, and Cypress.”

Comment #10: line 259: *What is the range of “Zone B”? Could it be shown on Fig 9-7B?*

Response #10: Text will be added to the caption of Fig 9-7B to explain that Zone B releases can be made when the lake stage is between the regulation schedule line and 48.5 ft.

Comment #11: line 246-267: *While not explicitly stated it appears the new Headwaters Revitalization Schedule has not yet been implemented even though all the construction of the S65 gate (and apparently all up-gradient improvements) has been completed. Why has the schedule not yet been implemented and when does the District anticipate starting that schedule?*

Response #11: The Headwaters Revitalization Schedule will be implemented after all construction is complete, which is currently scheduled for 2015. The Corps of Engineers will not authorize implementation of the new schedule until construction is complete. Also, there are several minor real estate issues that need to be settled before implementation.

No change in the chapter text is needed.

Comment #12: line 341: *Low dissolved oxygen occurred after the high rainfall event of July and it is suggested that this has occurred on occasions where high rainfall follows droughts. What is the mechanism for the loss of oxygen following these events?*

Response #12: We are studying this problem in FY13. We hypothesize that the mechanisms involved likely include some combination of the following: (a) dilution of oxygenated river channel water with anoxic water from runoff and upstream discharge (e.g., C-38 in Pool A); (b) influx of organic material from the floodplain and tributaries causing a rapid increase in BOD; (c) effects on photosynthesis including flushing of photosynthetic aquatic organisms by increased flow, and attenuation of light by increased water depth and turbidity; and (d) groundwater. We will add this statement to the discussion on p. 9-15 of the chapter.

Comment #13: lines 353-359 and Fig 9-8: *One assumes that the desired water level in PC61 (and all other piezometers) is to be above the ground surface for at least 180 day (Expectation #4), but is there a schedule for that period of inundation?*

Response #13: There is not a schedule for inundation per se. Water level at PC61 (and other stage recorders outside of the backwater effect of the downstream spillway) fluctuates primarily in response to changes in discharge. When the headwaters schedule is implemented, discharge at S-65 will vary with lake stage so that the largest discharges will occur near the end of the wet season and decline over the dry season. Seasonality of discharge is being evaluated with Expectation #2. No changes to the chapter text are needed.

Comment #14: line 440: *The hydrology section with each expectation listed and the degree to which it has been met discussed is a very nice approach; all parts of this section would have benefited from this layout.*

Response #14: We provided a statement of the expectation for those status reports that have expectations. We will revise the other status reports so that they follow the same format. We will begin these reports with a statement saying that no expectation exists for that study and explaining the purpose of our monitoring.

Comment #15: lines 550-588: *It is not clear why DO was not sampled with depth in the reference streams but was during the baseline period within stream segments of pools A and C. Is it because mixing due to flow makes for a more homogeneous DO profile in the reference streams, a mechanism not available in the stagnant pre-restored stream segment of the Kissimmee River? However, if this is true, why do expectations for DO include a depth profile or why is a depth profile apparent in the restored segments of Pool C that contain flowing water?*

Response #15: The lack of a depth profile in the reference stream data is one of the limitations in our reference data, although some of these streams are very shallow. We are collecting additional data in fiscal year 2013 from the reference streams to strengthen our dataset, and depth profiles are being considered. Depth profiles are included in the baseline and post-construction sampling because we observed changes in DO with depth in the stagnant channelized system, which we hypothesized would disappear or lessen after flow is restored.

No change to the chapter text is needed.

Comment #16: line 550-588: *There is clear improvement of DO concentrations in the post-restored Pool C reaches, but the improvement appears to be more dramatic in the dry season while the most critical times for fish health in during the wet season. Much of this improvement might be due to simply have flowing water in the channel, rather than some improvement due to a period of inundation. As mentioned in the general comments section, the compatibility of expectations may need to be addressed.*

Response #16: Please see our response to comment # 2. We recognize the importance of understanding the causes of low DO events in case some type of adaptive management can be implemented to reduce their severity and frequency.

No change to the chapter text is needed.

Comment #17: line 607: *It has never been clear why there is no explicit expectation with respect to phosphorus. What is the rationale?*

Response #17: A draft expectation for post-restoration TP concentrations was prepared several years ago. To support this expectation, a simple model of phosphorus movement through the river and floodplain was developed, but the estimates of potential phosphorus loading reduction were accompanied by a large amount of uncertainty. Consequently, staff decided not to link the phosphorus metric to a quantitative expectation because:

- (1) The restoration project is intended and designed to restore ecological integrity, not to reduce phosphorus loading. Assigning a target phosphorus load to the restoration project as an indicator of restoration success could have been confused with the project's purpose
- (2) The amount of reduction that might occur could not be predicted with an acceptable amount of uncertainty.

However, we acknowledge that river restoration may result in a substantial benefit to phosphorus control. For this reason, we have devoted significant effort to phosphorus monitoring and related studies.

No change to the chapter text is needed.

Comment #18: lines 664-710 and Figures 9-17 to 9-20: *Since the purpose of these graphs (based on the discussion) is to evaluate the long term trends in N and P concentrations it would be beneficial to have five groups of bars where each group represents a specific location and each bar represents a year. As currently presented the most obvious comparison is the general increase in load (and concentration) as water flows through the lower basin, which really is an intuitive conclusion (at least for load). Considering the number of bars in the proposed format why not use a line graph with year as the X axis?*

Response #18: Prior to the 2012 SFER, this information was presented in the way the reviewers suggest, with five groups of bars where each group represented a specific location. However, the author prefers the current format where each group of bars represents a year. This format allows easy comparison of data at each location for a particular year. The nutrient loads do not always increase from upstream to downstream, as demonstrated notably by the phosphorus loads for WY2012, and when this happens it can signify something important. The current format also allows a better view of year-to-year trends, which for loading data accounts for more variability than differences between locations.

The author has tested a number of other ways to present this information including the suggested line graph, which was too messy. The current bar graph is the best he has come up with, but as more years are added, this graph will become harder to view. The author will consult with some graphic experts to see if something better can be produced in next year's SFER.

Comment #19: line 780: *The survey of sediment and soil samples for phosphorus analysis is very important and it should be a priority to complete the analysis and reporting of this work in the coming year.*

Response #19: We agree. The final report for this survey is in the last stage of revision and will be available soon. In FY13, we will be incorporating the results into planning for the next phase of work.

No change to the chapter text is needed.

Comment #20: lines 854-856 and 893-894: *A formatting error has caused double printing.*

Response #20: These PDF conversion errors will be corrected.

Comment #21: lines 852-934: *There was a dramatic decrease in wading bird abundance in 2008. Was this a response to a hurricane? A 3-year moving average (Table 9-7) loses some information by minimizing this drop. While a small decrease is apparent in waterfowl numbers it is wading bird numbers that changed most.*

Response #21: I see the dramatic decrease in wading birds in 2007 (not 2008), which was a severe drought year. The floodplain was only briefly inundated that year during rains from Tropical Storm Ernesto in September, after which time the floodplain was completely dry throughout the entire dry season. There was little to no suitable foraging habitat that winter for either wading birds or waterfowl, so numbers were extremely low and comparable to baseline conditions. See the 2008 SFER for further details at www.sfwmd.gov/sfer.

Bird numbers have only slowly recovered since then, perhaps due to the negative impact the drought had on the prey base population of small fish and aquatic invertebrates, although we have no empirical evidence of this from the river. We are currently tracking the wading bird and waterfowl prey base in an attempt to answer this question.

No change to the chapter text is needed.

Comment #22: Figure 9-23: *Why was an unmistakable increasing trend in the first few years after rehabilitation (2002-2006) dramatically lost in all subsequent years? Was this a response to a major event? The data in this figure don't seem to reconcile with the data in any of the tables.*

Response #22: See response to Comment #21 above regarding the severe drought of 2007.

Comment #23: lines 903-906 versus 923-924: *The ranking of relative species abundance is inconsistent between these locations.*

Response #23: Lines 903-906 are referring to this year's data, and lines 923-924 are referring to all the post-restoration data since 2001. We can add the words "since restoration began in 2001".

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 10

Christopher Buzzelli, Peter Doering and Lesley Bertolotti

Level of Panel Review: Accountability

Reviewer: V. Novotny (AA)

Posted: 10/3/12 @ 10:12 AM EST

***General Comment 1:** This WY2012 Chapter 10 is possibly the first comprehensive, cohesive and highly professional chapter on the estuaries the district has presented to the public and this reviewer had an opportunity to see. The chapter is focusing on two important estuaries, the St. Lucie (SLE) and Caloosahatchee (CRE) Estuaries.*

General Response 1: We are extremely grateful that our approach to Chapter 10 was received favorably by the reviewer.

***General Comment 2:** In general, the chapter is well written and the flow of writing is logical and relatively easy to follow. The authors should be commended for this. A minor and rectifiable problem is the use of acronyms which are only sparsely identified or not identified at all. The authors must make sure that the acronyms are periodically defined throughout the chapter. The second problem, as it is typical for some but not all other chapters of the 2013 SFER, and the previous Water Year reports, is mixing of metric (SI) and US units and switching between the US and metric units without conversions.*

General Response 2: Great efforts were made by the Chapter authors, technical editor, and coordinator to define and clarify all acronyms in Chapter 10. We acknowledge that the mixing of traditionally applied US units with more acceptable SI units is scientifically and grammatically clumsy. This situation will be resolved through a switch to SI units prior to generation of Chapter 10 for the 2014 SFER. All rates and concentrations were reported using negative exponent format (i.e. mg L⁻¹). Text was added to direct reader to the front matter of the entire SFER document.

***Comment 1:** Page 10-1 to 10-4. Both estuaries in WY 2012 were hydrologically impacted by reduced discharges from Lake Okeechobee. The lake exhibits higher phosphorus and nitrogen concentrations and has been eutrophic. As a result of the reduced release the N and P concentrations in both estuaries dropped, not a result of BMPs but simply by shutting off the lake source of nutrients. This has positively impacted the oyster densities. The fresh water discharges are important because the section of the estuary affected by them is apparently the most productive part of the estuary.*

Because there is little information on the effects of low releases and reasons for them and connectivity to the Everglades system which are also fed by the Lake Okeechobee discharges, the district is developing the Adaptive Protocol. The legislation also requires development of the Watershed Protection Plans for the estuary watersheds. Each plan will have three components: (1) Pollutant source control program; (2) The Construction projects, (3) Research and water quality monitoring program. These programs appear to be similar to the other watersheds which drain into Everglades.

For the monitoring and assessment the authors divided the water year into dry (November and April) and wet (May to October) seasons. It could be pointed out that the SFWMD water year (WY) timing is similar to standard hydrological year which starts in October.

Response 1: This is an accurate synopsis of the Chapter 10 outline. However, the use of WY (May 1 -April 30) does not appear to be consistent with a hydrological year that begins in October. WY was defined in the last paragraph of the Introduction on page 10-4.

Comment 2: Page 10-7 lines 251 and 293/294. Provide conversions between US and SI units (in parentheses) for cfs and m³/s and acres and hectares.

Response 2: There are 31 occurrences for “cfs” throughout the document. All inflows utilize this base unit. Below is the text from page 10-4 that introduces nomenclature and categories with text added to facilitate conversion to SI units.

“The structure and content of the SLE and CRE sections of this chapter are identical with summary information on watershed rainfall, freshwater discharge to the estuaries, salinity distributions, total nitrogen (TN) and total phosphorus (TP) loads, estuarine water column concentrations, patterns of SAV community composition, and the status of oyster reef habitat. Monitoring data from both estuaries were summarized by water year. A water year is the period of record (POR) from May 1 of one year to April 30 of the next (WY2012 began on May 1, 2011 and ended on April 30, 2012). The categorical variable “season” was defined by splitting the months into dry (November–April) and wet (May–October) groupings for all calculations. **Short-term freshwater inflows are in units of cubic feet per second (cfs) where 1 cfs = 2445.1 m³ d⁻¹. All spatial references are in acres where 1 acre = 0.405 hectares. Annual inflows are in units of acre-feet per year (AF y⁻¹) where 1 AF = 1233.5 m³, and, 1 foot = 0.3048 m. The standardized units and definitions for the entire SFER appear in the document front matter. Salinity is derived from a dimensionless ratio and therefore has no units in reporting.** Monitoring data were graphed in time series format over the past three water years (WY2010–WY2012) to examine recent intra- and inter-annual patterns. Three timescales were used to summarize by water year in tabular format (long-term, WY2010, WY2011, and WY2012). Long-term reporting (multi-annual to decadal timescales) depended upon data availability for the variable of interest. PORs were chosen to maintain consistency between the two estuarine systems. Values were summed (rates of rainfall, inflow, and loadings) or averaged (concentrations of salinity, TN, TP, SAV, and oysters) by water year and season in order to compare and contrast among the three timescales.”

Comment 3: Pages 10-7 and 10-8 list local water quality and restoration projects in SLE watershed. In comparison to the total area of the watershed these projects are minor and some, such as sewer connections and dredging for navigation would have been done with or without the Everglades Program. Provide conversions of feet to meters and acres to hectares in parentheses. These projects have not been completed yet.

Comment 4: Pages 10-8 till 10-10 list four construction projects in the CRE watershed. One is an aboveground reservoir (provide conversion from ac-ft to m³). A similar (same?) project evaluated in Chapter 4 reported very low efficiencies of such reservoirs. The second project is conveyance attenuation of pollutants in a creek and ditch. The third project is to test strategies for nitrogen and other pollutants removal using wetlands, focusing on removal of dissolved organic nitrogen (apparently ammonium). The estuary is nitrogen limited as it was proven by a TMDL prepared for the watershed. The fourth project is the development of the Caloosahatchee basin storage/treatment alternatives. All four projects are in their initial phases of implementation, some are waiting for the funding to become available.

Response 3-4. The aboveground reservoir in chapter 10 is quite different than the aboveground reservoirs/impoundments evaluated in chapter 4. The reservoir in chapter 10 is the large regional reservoir (CERP Caloosahatchee River West Basin Storage Reservoir) that will provide 170,000 acre-feet of water storage capacity to capture and store basin run-off and lake releases. It will also help maintain a more desirable salinity balance in the Caloosahatchee estuary. The primary purpose is not water quality although that will be a ancillary benefit. The aboveground impoundments evaluated in the BMP demonstration grant work discussed in Chapter 4 are farm scale BMPs to capture run-off from individual farms.

Comment 5: Pages 10-10 to 10-23 describe the SLE hydrology, water quality and aquatic habitat monitoring program. This program has been apparently initiated recently because the last year report provided only a cursory mentioning of the monitoring program and reported mostly the status of grasses. This report was withdrawn. This chapter on the other hand provides an excellent delineation of the program and presentation of preliminary results. As more data will be gathered this program has a potential to become a gold mine for marine scientist and will enable the SFWMD and state planers to identify quantitatively the stressors and their impact and develop good plans. The TMDL for the SLE watershed has been already prepared and the rule was adopted by the state. The TMDL goal for the SL estuary is 0.081 mg/L of Total P and 0.72 mg/L of Total N, respectively. One may argue that the allowable P load is high and would not guarantee a good status (better than eutrophic) of water quality in the estuary. However, the subsequent monitoring results show that the estuary is nitrogen limited rather than phosphorus limited; hence, both N and P should be reduced but N reduction may provide more immediate results. The studies also identified salinity limits that will dictate the fresh water releases from Lake Okeechobee. The salinity has apparently a significant effect on oyster densities.

Response 5: We thank the reviewer for the supportive comments about Chapter 10. Laboratory bio-assays were conducted to ascertain that the SLE is largely N limited, although P can be important depending upon the inorganic vs. organic fractions and ratios of the incoming N and P loads. Examination of nutrient ratios and bio-assays suggest that the CRE is nitrogen limited in most places at most times, although P-limitation may occasionally occur. While not the only factor that affects oyster population dynamics, salinity is an important determinant for the survival and status of the eastern oyster, *Crassostrea virginica*.

Comment 6: Page 10-15 needs conversions from inches to mm (millimeters) or cm. Although previous section already identified dry and wet season (November to April and May to September) this identification should also be included into Table 10.1. The chapter correctly states that annual rainfalls are also affected by long term climatic changes (El Niño). However, have any projections been made on the effects of the ongoing long term climatic changes on the future rainfalls in the South Florida region?

Response 6: A parenthetical conversion was added to the text (0.0-7.1 cm d⁻¹). The SFER is intended to summarize environmental patterns over the past 3 water years. Thus, there no future rainfall projections were included.

Comment 7: On page 10-16 clean up the units in Table 10-2. Since four columns report the results in metric tons (mt) change million acre-ft to 109 m³ (106 acre-ft = 1.233 Km³). Note that 1 Km³ = 109 m³. Provide conversion between the US and SI units. Provide conversion from cfs to m³/sec on Figure 10-5 and throughout the rest of the chapter.

Response 7: Units of acre-feet y⁻¹ and mt are the standard for the SFER. The table title was edited in the following way: “Total freshwater inflows in 106 acre-feet (ac-ft) y⁻¹ and total nitrogen (TN) and total phosphorus (TP) loads in metric tons (mt) y⁻¹ to the St. Lucie Estuary (SLE) from three structures (S-80, S-48, and S-49) and Lake Okeechobee for the long-term average for WY1996–WY2012, WY2010, WY2011, and WY2012. 106 ac-ft = 1.2 x 109 m³.”

Comment 8: When referring to salinity provide units (even though salinity may be dimensionless such as PSU). A sentence such as “... a preferred salinity envelope of 8-25 has been” does not make much sense to those outside of the estuary team of SFWMD. Flow unit cfs on Figure 10-6 is usually not capitalized unless all lettering is in capital letters. Provide a conversion to SI units.

Response 8: Please see sentence at beginning of page 10-5: “Salinity is derived from a dimensionless ratio and therefore has no units in reporting (Millero 2010).” The axis label and figure caption were edited to lower case “cfs”, and, to include the conversion to m³ d⁻¹, respectively.

Comment 9: Page 10-18 and 10-19. Provide salinity units in Table 10-3. Table 10-4 and Figure 10-7 indicate that (a) phosphorus concentrations are very high that would normally be in eutrophic or even hyper-eutrophic range, (b) luckily the estuary is nitrogen limited. Figure 10-4 shows that algal blooms might have occurred in the past WYs as documented by spikes of high Chl-a concentrations.

Response 9: Please see sentence at beginning of page 10-5: “Salinity is derived from a dimensionless ratio and therefore has no units in reporting (Millero 2010).” As previously mentioned, laboratory bio-assays support the assertion that the SLE is largely N limited, although P can be important depending upon the inorganic vs. organic fractions of the incoming N and P.

Comment 10: Page 10-20. What is POR? The definition of this acronym was not found. It is a good idea to identify rare acronym more than once in the chapter. The chapter also reported at several places that reducing fresh water inputs into the estuary improved oyster densities. This improvement is not seen on Figure 10-8 which shows decreasing densities in the last three years.

Response 10: A POR, or period of record, was defined at the end of the Introduction on page 10-4 with other Chapter reporting conventions. While oyster densities have been stable or increased depending upon the particular sampling site over the past 3 WY's, the condition index (CI) appears to have declined over the same POR.

Comment 11: Page 10-22. Figure 10-8 also contradicts the “Significant Findings” on page 10-22, reporting increases of oyster densities. The fact that the estuary is overloaded with P but is limited by nitrogen could be added to the findings.

Response 11: Please see Response 10 for clarification on oyster densities. The magnitude and nature of N vs. P responses by the SLE are more complicated than simply comparing loading and internal concentrations.

Comment 12: Pages 10-24 to 10-37 describe the same programs for the CRE.

Response 12: The formats and contents for the SLE and CRE components were intended to be identical to ensure consistency and clarity for the reader.

Comment 13: Page 10-26 lines 729-730 has acres (US) and m² (SI) next to each other. It is suggested to use hectares instead of acre with conversion to acres in parentheses.

Response 13: This is a difficult sequence because the field sampling grid is 3m x 3m but habitat extents are reported in acres. Thus, the text was edited:

“There are seven sites (1, 2, 4, 5, 6, 7, and 8; Figure 10-12, bottom panel) with SAV meadow sizes ranging from 1.0-2.0 acres (0.4-0.8 ha) along the length of the CRE from WY2010 through WY2012. A large quadrant grid (3 m x 3 m = 9 m² = 0.0009 ha) subdivided into 25 equal sub-quadrants was deployed at randomly selected locations within each of the seven sites.”

Comment 14: Page 10-28. Provide conversion of inches to cm or mm. Again it would add clarity if the months of wet and dry season are included in table 10-5.

Response 14: Unit conversions were added to text with dry and wet seasons re-defined in the Table caption.

Comment 15: Page 10-29. Same issue with conversions from million acre – ft to more convenient Km³ or 10⁹ m³ and cfs to m³/sec, add units for salinity.

Response 15: The Table caption was edited to include 10⁶ ac-ft = 1.2 x 10⁹ m³. As discussed, salinity is a unitless value.

Comment 16: Pages 10-30 to 10-32. Lake Okeechobee was found to be a major source of nutrients also in the CRE watershed. In Table 10-8 the WY 2012 has the smallest Lake Okeechobee water release and, accordingly, the N and P loads were small (there is an issue with units). Figures 10-16 and 10-17 and Table 10-9 show chlorophyll-a concentrations between

oligotrophic and mesotrophic water quality. The Chl-a spikes into mesotrophic/low eutrophic water quality are related to increasing phosphorus levels which could lead to concluding that the estuary is P limited. The N/P ratio is also higher than that for SLE. The releases of the eutrophic Lake Okeechobee water may be the cause of elevated Chl-a concentrations. Meaning of the acronym POR on line 845 should be identified.

Response 16: Table 8 caption was edited:

“Total freshwater inflows in 106 acre-feet (ac-ft) y-1 and total nitrogen (TN) and total phosphorus (TP) loads in metric tons (mt) y-1 to the CRE and the total and contribution of Lake Okeechobee to the TN and TP loading to the estuary for the long-term average (WY1996–WY2012), WY2010, WY2011, and WY2012. 106 ac-ft = 1.2 x 10⁹ m³.”

A POR, or period of record, was defined at the end of the Introduction on page 10-4.

Comment 17: *Pages 10-38 to 10-54 describe the Adaptive Protocol Study. The goal of the study is to develop understanding of the Lake Okeechobee releases on the quality of the estuaries. The study begins with the formulation of the hypothesis expressed on Figure 10-39. Then it describes in-situ monitoring and cruises and present preliminary results. The model presented on Figure 10-20 specifies that the most productive sections, the most intensive primary productivity, occur in the fresh water/brackish sections of the estuary. The primary productivity is phytoplankton algae and plant growth which are grazed by zooplankton and progressing to higher trophic levels, i.e., fish and fish eating fowl and mammals. Lines 948 to 950 emphasize that increased rates of primary production may not be manifested as increased biomass (increased Chl-a concentration) because of predation by zooplankton and filter feeders. Harmful Algal Blooms (HAB) occur when the balance between primary production and predation is disrupted.*

On page 10-38 define LSZ.

Response 17: This is an accurate synopsis of the AP Study. It is important to remember that a surplus of phytoplankton-derived organic matter does not necessarily denote a HAB. Maximum CHL concentrations may be neither “harmful” nor a “bloom”. The low salinity zone or LSZ was re-defined on page 10-38.

Comment 18: *Pages 10-40 to 10-44 describe methods. The scientific reasoning for pulse investigation described on page 10-40 is not clear. What was obtained by these investigations? Also report pulses in m³/sec. On page 10-42 provide a conversion from knots to km/hr (note that all distances in this chapter are reported in kilometers) and miles/hr (in parentheses). On page 10-45 add units of salinity. On the same page, the reported increased salinity was caused by small discharges from Lake Okeechobee.*

Response 18: The rationale and background information for the AP Study were explicitly described in the Introduction on page 10-38. Results and significance were contained on pages 10-52 to 10-55. As discussed, salinity is a unitless value. The text was edited:

“15.2 km h⁻¹ and an average distance of 15–26 m (0.015-0.026 km) between surface water recordings.”

Comment 19: *Table 10-11 page 10-46. This table needs salinity units. The DO maxima in the fifth column must be in error, 3.8 mg/L is probably not a maximum but a minimum. The other values ranging from 15 to 19.7 mg/L would represent a gross oversaturation which is erroneous based on the CHL-a concentrations reported and mean and standard deviations in the table.*

Response 19: The table caption and column heading were edited for clarity. The last column in Table 10-11 is the distance of the maximum concentration in km from S-79 and not the concentration. As discussed, salinity is a unitless value.

Comment 20: *Figure 10-47 presents a sharp increase in turbidity in the downstream sections of the estuary even when the concentration of Chl-a was decreasing. Page 10-48 attributes the turbidity increase to waves stirring sand by boat traffic and wind. In this figure salinity is*

expressed in psu (for the first time in this chapter?). It should be reported so through the chapter. The legend on Y-axis should report chlorophyll as Chl-a, not chl-a.

Response 20: Chlorophyll *a* concentrations and turbidity are independent variables measured using different methods. While Chl-*a* determination results from differential absorption, turbidity is relative measure of light scattering off inorganic particles. The greatest turbidity values observed at the oceanic end in San Carlos Bay are due to the greater percentage of sandy particles that easily resuspend with wind waves and boat wakes. The reviewer was correct, the y-axis for the salinity and chlorophyll *a* distance plots on Figure 10-24 were edited for consistency.

Comment 21: *Table 10-12 and Figure 10-26 on pages 10-50 and 10-51 indicate that, unlike SLE which is clearly nitrogen limited, the CRE might be phosphorus limited. Has it been clearly established? The blue Chl-*a* spikes to mesotrophic/eutrophic range at three upstream sections (stn 1,3,5) on Figure 10-51 correspond to blue P spikes, hence, CRE may be phosphorus limited. The authors on page 10-54 also attributed the elevated chl-*a* concentrations to hydrologic conditions and stratification. Apparently, there is a great potential for algal blooms when there is not inflow from Lake Okeechobee and temperature is greater or equal 27°C.*

Response 21: The nutrient status and potential for N vs. P vs. N:P limitation have not been sufficiently quantified or established for the CRE. Observations and data analyses do support that a combination of hydrologic conditions and water temperature can account for periodically elevated chlorophyll *a* concentrations.

Summary Comment 1: *In general, the chapter presents a credible and comprehensive plan of monitoring that would lead to an efficient management of two estuaries and development of Adaptive Protocols for releases of flow from Lake Okeechobee which is their main source of water and nutrients. The lake is currently overloaded by phosphorus. A complete stoppage of Lake Okeechobee inputs is apparently not possible. The fresh water input is needed to stimulate primary productivity but the stimulus must be balanced and not results in phytoplankton blooms and disappearance of species, grasses and habitat.*

Summary Response 1: We thank the reviewer for an insightful and thoughtful synopsis of the Chapter.

Summary Comment 2: *It was pointed out by the reviewer in his Chapter 4 review document that the high phosphorus loads to the lake may be, in addition to the “traditional” agricultural and urban sources, caused by the effects of the Bone Valley phosphate mine which appears to be in a dangerously close vicinity of the Kissimmee River watershed, which is the main flow and nutrient source to the lake and might also be one of the sources of the atmospheric P deposition to the lake, estuaries and Everglades. This is an observation; nevertheless, it implies that the reduction of phosphorus inputs may be a long term goal rather than a short term possibility.*

Summary Response 2: We fully agree with the reviewer.

Summary Comment 3: *Regarding the specifics of Chapter 10, the results of monitoring agreed with the conceptual model depicted on Figure 10-20 and the monitoring effort and protocol development are on the right track and have already brought good quality data. It has been already established that SLE is nitrogen limited, hence, less sensitive to phosphorus discharges from the lake and the watershed. CRE may be more sensitive to phosphorus loads and there is greater potential for harmful algal bloom development in the upper reaches of the estuary. Extreme eutrophic concentration of Chl-*a* exceeding 60 µg/L were observed in the upper reaches of the estuary. These findings and hypotheses may lead to somewhat different protocol for controlled water discharges from the lake.*

Summary Response 4: Once again we acknowledge and thank the reviewer for an accurate synopsis of Chapter significance. Furthermore, the potential for differential nutrient limitation within and between the two estuaries warrants much more investigation and experimentation.

Summary Comment 5: *The authors of the chapter suggest that their future monitoring should expand and include fish larvae as a biological end point. This is commendable but from a managerial stand point, the monitoring must produce also data for development, calibration and verification of computer models of the estuaries that, in addition to further TMDL N and P load refinement, would lead to a (real time?) management model for operators regulating the discharges from the lake and proportioning them between the storage in the lake, releases into the two estuaries (discharging to Atlantic Ocean and Gulf of Mexico) and the Everglades which need the lake waters too.*

Summary Response 5: The reviewer was once again on point. The Coastal Ecosystems Section at the SFWMD has developed a series of seasonal DIN and DIP budgets for both estuaries from 2002-2008. From these analyses a full water quality model is being developed for the CRE to link hydrodynamics, nutrient loads and N and P cycling, phytoplankton dynamics, gradients of submarine light extinction, and the survival and growth of both seagrasses and oysters in the polyhaline estuary.

Summary Comment 6: *It is hoped (expected?) that in the next 2014 report the district scientist will also report on other important coastal water bodies such as Biscayne Bay.*

Summary Response 6: Chapter 10 will continue to highlight the St. Lucie and Caloosahatchee Estuaries to fulfill the reporting requirements of the Northern Everglades and Estuaries Protection Plan (NEEPP) legislation. In addition to the NEEPP, the Coastal Ecosystems Section's program will focus on two elements of the District's core mission: operation of District infrastructure and quantifying environmental water supply to support natural systems. Progress on these fronts will be reported in the SFER and may include:

Implementation of the Lake Okeechobee Regulation Schedule and Operations

- Results of field studies to determine effects of pulsed, low level releases of freshwater to the St. Lucie, Caloosahatchee, and Loxahatchee Estuaries
- Progress on developing a statistical position analysis to forecast of salinity in the Caloosahatchee and St. Lucie Estuaries

Water Protection Rule Development

- Report on technical work conducted to support reservations of water for the C-43 Reservoir and Biscayne Bay Coastal Wetland CERP Projects
- Report progress on the 2017 update of the Caloosahatchee Minimum Flow and Level
- Report technical progress on the Naples Bay Hydrodynamic Model